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GAILLARD'S MEDICAL JOURNAL.

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ORIGINAL ARTICLES.

SOME ILL EFFECTS OF THE CORSET.*

By EUGENE LEE CRUTCHFIELD, M.D., Baltimore, Md.

Nearly every one will concede that there is no greater tyrant than Fashion. To please this goddess, men and women (even when possessed of intelligence) will endure any discomfort, distort the artistic proportions of the figure designed by Nature, sacrifice morality, and jeopardize health. The constricted waist of woman exhibited on all occasions and the décolleté costumes generally worn when evening dress is required, attest the truth of this assertion. Both of these produce discomfort, rob the figure of its graceful outlines, suggest immoral thoughts and desires, and induce many serious maladies. It is, however, only to some of the injurious effects of the narrow waist that I now desire to call your attention. As Dr. G. W. Hambleton, president of the Polytechnic Physical Development Society of Great Britain, has said in his valuable brochure on "The Suppression of Consumption," "Fashion dictates the size and the shape of our clothes, and our bodies have to and do conform thereto. A beautiful example of this is seen in the hideous distortion of the lower part of the chest produced by wearing a corset that never, never is tight."

Before considering the evil results of this article of female apparel, let us examine one argument so often advanced in its favor. Women who are accustomed to wear a corset tell us that if they leave it off they feel as though they would drop to pieces. Hence they illogically conclude that the corset is a beneficial support. So far from this being true, this weakness proves that their muscles, depending upon the assistance seemingly afforded by stays, have lost their tonicity and have ceased to develop. The non-usance of any other structure in the body would produce the same effect and call for artificial help. An intelligent lady once told me that she herself could not leave off the use of corsets, but if the rearing of any girls were entrusted to her care she would never allow them to put on stays. In other words, she realized the detrimental effects of the corset, but in her own case the muscles had so long leaned upon this support that they had become weak. Thus we see that what women frequently adduce in favor of the corset, is in reality an argument against its employment. It is always injurious to give too much support to any part of the body. On the contrary, it should be made to develop and grow strong and independent.

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* Read before the Baltimore Medical Association, April 26, 1897.

We naturally look for a manifestation of the deleterious consequences of the use of the corset in that part of the body upon which it is worn. Nor are we disappointed. Although you can never find a woman who will acknowledge that her stays are tight, it is safe to say that not one in a thousand breathes correctly. Nearly every woman employs only the clavicular mode of respiration, instead of the diaphragmatic, as intended by Nature. The latter is the method practiced by men. That both sexes should breathe in the same manner, is proved by the following facts: 1st. Animals, both male and female, breathe deeply. 2d. Children, both boys and girls, employ diaphragmatic respiration up to a certain age. If proper attention is paid to their clothing, to see that their waistbands do not become too tight, individuals of the masculine persuasion continue to breathe correctly throughout life. Girls do so until the time for putting on the corset, and then farewell, a long farewell, to correct breathing, and, in many instances, to the enjoyment of health. 3d. Savages of both sexes, unfettered by the restrictions of fashion, breathe through the whole extent of the lungs, allowing the diaphragm to descend and press the abdomen downward and outward during inhalation. Some text books on Physiology, however, erroneously state the contrary of this. Dr. Thomas J. Mays, of Philadelphia, has carefully investigated this point by studying the respiratory movements of eighty-one Indian girls in the London Institution in that city, using for this purpose the pneumograph. In each case he took both a costal and an abdominal tracing. While all of these girls were Indians, some of them had a large admixture of white blood in their veins. By his investigations, Dr. Mays discovered, *a*, that in full-blooded Indian girls the abdominal is the only type of breathing; *b*, that "those who showed a costal type, or a divergence from the abdominal type, came from the more civilized tribes, like the Mohawks and Chippewas, and were either one-half or three-fourths white;" *c*, "that the costal type of the civilized female is developed through the constricting influence of dress around the abdomen;" *d*, "that the costal type of breathing is much less pronounced, or may be absent, or may even revert to the abdominal type, in those civilized women who never wore corsets, or practiced tight lacing of any kind;" and, *e*, that by placing around the abdomen a broad band sufficiently tight to interfere with the action of the midriff, there may be produced a modification of the chest movements of the civilized male, i. e., that he will acquire the costal mode of respiration. Dr. Mays published an entertaining and instructive article on this subject in "The Century Magazine," of August, 1893. My fourth reason for believing that abdominal respiration is the proper mode for both sexes is that the educated adult breathes thus—when asleep. Then, too, in my professional attendance upon the ill, I have frequently availed myself of the opportunity of observing the breathing movements of women when divested of their stays. On such occasions I have noticed that they breathed diaphragmatically.

Incidentally I may remark that one of the best methods ever proposed for resuscitation in cases of asphyxia neonatorum is that suggested by the late Prof. Harvey L. Byrd, A.M., M.D. I have no doubt that the reason of its efficiency consists in the fact that it acts in accordance with the theory of diaphragmatic respiration.

Such well-known medical authorities as Lennox Browne, of London; Prof. A. Lapthorn Smith, of Montreal, and the late Prof. H. Newell Martin, of Baltimore, have advocated deep, abdominal breathing for women. Prof. Howard A. Kelly says that "corsets prevent the proper expansion of the lower lungs, even though they may not unduly confine them according to the opinion of the wearer." He says: "One of the most important results from bicycling for women, in my opinion, is that it will require them to do away with the corset."

All vocalists who can lay claim to the title of artiste contend that deep breathing is the proper mode for women as well as for men. I have letters from Mesdames Jenny Busk-Dodge and Luisa Cappiani (both of whom are recognized by musicians as being authorities on all questions pertaining to vocal culture), in which they claim that men and women should breathe alike. Some have agreed that pregnancy would constitute an obstacle to diaphragmatic respiration. This is most emphatically denied by Mme. Cappiani. Mme. Julie Rosewald, in her excellent little book entitled "How Shall I Practice?" (which was written for female students exclusively), says, "Breathe naturally, like a man or an infant whose ribs have never been compressed by stays."

The following extract is also of peculiar interest, since it gives the opinion of one who can overcome the prejudice of her sex and thus view this subject in the light of science and common sense. It is here taken from "The Maryland School Journal," of May, 1877, which copied it from "The Ohio Educational Journal." Mrs. M. Josephine Warren says: "The common fault in breathing, especially among women and girls, is to breathe from the upper part of the chest and not from the waist. An animal breathes from the flanks, and nature dictates that human beings should breathe from the lower part of the chest and waist. Of course this implies room at the waist for exercise of the different sets of muscles which control the breathing. Steel supports, whalebones, bands, and strings impede natural breathing, hence are enemies to health and good voice. Banish the corset and breathe deeply and fully, and you have taken the first grand step toward a round, full, pure voice. The thin, weak voices of a majority of girls are due to the feeble supply of breath, by which the circulation becomes imperfect and the whole system enervated."

At a meeting of this Association held December 18, 1893, I advanced a new theory as to the etiology of phthisis pulmonalis.* It was that "whether occurring in man or woman, consumption is largely due to faulty methods of respiration." In the female sex the corset, by preventing the expansion of the lower lobes of the lungs, throws additional work upon the apices (the most frequent location of the tubercular process) and subjects them to increased strain. As a consequence, the apices become tired, worn out, and exhausted. They are therefore unable to cope with the bacillus tuberculosis, which thus gains the victory and implants a disease that will eventually cause the death of the devotee to Fashion. Thus far I have had no reason to abandon or even to modify this hypothesis. On the contrary, my conviction has become strengthened that it explains the occurrence of phthisis in women. Statistics show that this disease is less frequent in the fair sex than in males, but there is reason to believe that it would become still rarer among women if they would abandon the corset.

Even, however, should phthisis pulmonalis not be thus developed, the faulty and imperfect respiration leads to defective aeration of the blood. It is impossible for a woman wearing corsets to inhale enough oxygen and ozone. The blood is thus insufficiently oxygenated. Spanæmia is the result with its train of innumerable symptoms. Every physician is fully cognizant of the cephalalgia, lassitude, imperfect circulation, pallor, and constipation, so common in women.

Again, cardiac palpitation is caused by the use of the corset. With truth it has been said that there is no superfluous space in the human body. The stay which crowds the lungs into too small a compass, also compresses the heart and forces it to perform its work against heavy odds, and the effort to do so manifests itself in palpitation. Then, too, were there no undue pressure exerted upon the heart itself by the corset, either directly or indirectly, the two functions, respiration and circulation, are so intimately related and dependent the one upon the other that it is impossible to interfere with the healthy activity of the organs of one without disturbing the physiological performance of the other. Furthermore, palpitation of the heart may be a reflex action caused by the effect of the corset upon the lungs and transmitted through the pneumogastric nerves and the sympathetic system. According to Dr. Thos. Shapter (in Quain's "Dictionary of Medicine"), with the several forms of cardiac palpitation "there may be the accompanying signs of choking—the globus hystericus; vertigo; tinnitus aurium; impaired vision, with a feeling of distention of the eyeballs; a copious secretion of the extremities; fear of death; partial unconsciousness; or actual syncope. Paroxysms such as these may be preceded by a somewhat prolonged state of cerebral disturbance, as evidenced by heat of brow and vertex, headache, and an inaptitude to think or regulate the thoughts; and, as there is generally a self-consciousness of the abnormal action of the heart, the anxiety on this account serves to impress the mind with so much fear and inquietude, as to tend to increase and prolong the disorder that has induced them." Truly, this is a formidable array of symptoms, but easily traced to the wearing of the corset.

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* *Vide* "Maryland Medical Journal," March 10, 1894.

Certain hepatic disorders are produced by means of stays. These are brought about both directly through pressure and indirectly as a result of the injurious effects of corsets upon the lungs and the heart. Dr. J. Milner Fothergill says, in his work entitled "The Maintenance of Health," that in tight lacing "the bust is driven out at the top of the stays in a most repulsive manner, the respiratory movements almost arrested, and the abdominal viscera squeezed till the indentations of the ribs upon the liver could be distinctly seen on examination after death." Although he mentions these as occurring from tight lacing, I must repeat what I have already said, that, "although you can never find a woman who will acknowledge that her stays are tight, it is safe to say that not one in a thousand breathes correctly." If the corset will have this effect even when the wearer considers it loose, it will also produce at the same time the evil consequences enumerated by Dr. Fothergill. H. Newell Martin said (in "The Human Body") that "in extreme cases of tight lacing some organs are often directly injured, weals of fibrous tissue being, for example, not unfrequently found developed on the liver, from the pressure of the lower ribs forced against it by a tight corset." According to W. Johnson Smith, Surgeon to the Seamen's Hospital, Greenwich, Eng., acquired malformation of the liver may be due to compression of the organ by tight lacing. (Quain's "Dictionary of Medicine.") Dr. Stephen H. Ward, consulting physician to the same hospital, says in the same work that "passive hyperæmia, to which the term mechanical is applied by some authors, is due to interference with the return of blood from the liver through the hepatic vein and inferior cava to the heart. Such interference may be due immediately to dilatation of the right heart, with affection of the tricuspid valve; to obstruction to the circulation in the course of the pulmonary arteries, caused by different diseases of the lungs, or, further on, in the line of the circulation, to disease of the mitral or aortic valves. The affections of the lungs which interfere with the pulmonary circulation are either acute, as pneumonia, and then the hepatic hyperæmia may pass off with the disease; or they are chronic, as emphysema and fibroid disease, and then the hyperæmia will persist. Mechanical congestion of the liver may result from direct obstruction to the flow of blood by the pressure on the vena cava of aneurismal or other tumors." We thus see that whatever impedes the portal circulation, will cause passive hepatic hyperæmia, and, consequently, the corset must be condemned.

The late Austin Flint (in his "Practice of Medicine") assigned pulmonary and cardiac obstructions to the circulation as ætiological factors in the production of chronic congestion of the liver. It has already been shown that stays are responsible for organic and functional derangements of the heart and the lungs. In a paper* entitled "What Civilization Is Doing for the Human Female," which Dr. A. Lapthorn Smith read before the Southern Surgical and Gynæcological Association, November, 1889, he claimed that the corset produces "congestion and enlargement of the liver, due to limited pumping power of the diaphragm. This is especially important, as it compresses the inferior vena cava as it passes up behind the liver, so that there is a damming back of blood in all the branches where they empty into it, causing passive congestion of the pelvic organs, and, in many cases, varicose veins." Since it often happens that in chronic hepatic congestion an insufficient quantity of bile is poured into the duodenum, the digestion which should have taken place is imperfectly performed, the absorption of fats is interfered with, and constipation is induced.

At a meeting of this Association held February 12, 1894, Dr. David Streett said that tight lacing is a cause of gall-stones. Such a statement need occasion no surprise when we consider the pressure exerted upon all the abdominal viscera and the consequent displacement to which they are subjected even in women who boast of the loose fit of their corsets. By compression of these organs not only the circulation is obstructed, but also the outflow of the natural secretions is impeded. The gall-bladder thus becomes distended, and in this way favors the other conditions, which are recognized as ætiological factors in the production of gall-stones. To elucidate this point more fully, I give the following extract from "The Medical Bulletin" (Philadelphia), of April, 1895, in which it is credited to the "American Practitioner and News:"

"According to Dr. Marchand, the relation of tight lacing to the development of gall-stones is rendered very clear by noticing the situation of the gall-bladder in the liver deformed by tight lacing. The furrow caused by lacing runs directly across the right lobe of the liver, as a result of which there is a tendency to atrophy of the gall-bladder. When tight lacing has been practiced to an extreme degree an artificial fissure is formed in the liver, giving rise to what is termed the 'lacing-lobe,' which carries with it the gall-bladder. The constricted portion of the liver is found to be just at the point of junction of the gall-bladder with its duct. In these cases, according to the author, it is common to find the gall-bladder greatly distended, extending far beyond the border of the liver, and frequently an examination made post-mortem reveals the presence of gall-stones. Stagnation of the bile is well known to be one of the most important causes of the formation of gall-stones. A change in the composition of the bile, from catarrh resulting from congestion of the mucous membrane and the thickening of the bile due to failure of the gall-bladder to evacuate itself completely, gives rise to the formation of small masses which serve as nuclei for calculi; hence anything which obstructs the free outflow of bile through the cystic duct must favor formation of gall-stones. Marchand is also of the opinion that many cases of cancer of the liver should be attributed to tight lacing. It is only a few years since Langenbuch was obliged to open an abdomen to remove a 'lacing-lobe' of the liver which had been so completely separated from the rest of the organ as to cause its death, rendering its removal necessary. In view of such facts as these, it is the duty of every physician to take special pains to warn his patients against the evil effect of this pernicious practice. Few women are conscious of the fact that they are injuring themselves by tight lacing. But physicians generally preach to deaf ears in an audience composed largely of votaries of fashion." W. Johnson Smith (from whom I have before quoted) corroborates Marchand's statement about the fissuring of the right lobe of the liver. He says: "At last the lobe is divided into two portions by a deep transverse groove, which portions are connected merely by a membranous band, composed of thickened serous membrane, and the corresponding portion of the hepatic capsule." Before passing to the next point, I will remark that it is a significant fact that gall-stones are more common in women than in men, and that Flint, quoting from Ziemssen's Handbuch, says that of 756 cases of hepatic cancer, 334 occurred in men and 422 in women.

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* *Vide* "Virginia Medical Monthly," December, 1889.

Constipation is very frequent in women. The fact has been pointed out that it is induced by defective aeration of the blood due to imperfect respiration and favored by too little bile in the duodenum caused by compression of the liver and the gall-bladder, but, independently of these conditions, it may be produced per se by the corset. Even when loosely adjusted to the figure, the stay, with its busks, forces the intestines into a smaller space than Nature allotted. Consequently, they have not room to perform their peristaltic movements. Constipation results, and the contents of the bowel undergoing putrefactive changes produce what Sir Andrew Clark called "fæcal anæmia." In this way the whole system becomes vitiated.

February 26, 1894, Dr. John T. King stated before the Baltimore Medical Association that "there is one case on record of floating kidney caused by tight corset, producing dilatation of the stomach." Flint says: "The causes of dilatation relate, in the first place, to mechanical obstacles to the passage of the contents of the stomach into the intestines." Among these mechanical means producing obstruction he mentions "the pressure upon the duodenum of a wandering right kidney." It is readily seen how the pressure exerted by the corset could displace an organ so loosely attached as the kidney. Flint gives the use of corsets as one of the causes of movable kidney. Then, too, like gall stones, it is much more common in women than in men. Ebstein found that in 96 cases the relative frequency was 82 in women and 14 in men. However, apart from the influence of ectopia venis, dilatation of the stomach could be produced primarily by stays compressing that viscus and thus preventing its contents from passing through the pyloric orifice. In like manner, dyspepsia is brought about, the stomach being so compressed that it cannot perform its normal churning movements.

The following paragraph from "The Medical Bulletin," of August, 1892, which obtained it from "The British Medical Journal," describes an unpleasant and mortifying effect of stays: "L'Estomac et le Corset.—Dr. Chapotot, in a recent essay published by Baillière under this title, refers to a matter of some interest to ladies. Young women have often reason to complain of disagreeable noises caused by air moving about in the epigastric region. Our author attributes these noises, which differ from borborygmi, to a vertical bilobulation of the stomach, caused by the pressure of stays. During expiration the upper lobe is relieved of pressure by the ascent of the diaphragm. The lower lobe is, on the other hand, subjected to great pressure from the abdominal muscles. Hence, air and liquids are forced upward into the upper lobe, through the narrow isthmus produced by the pressure of the stays; as they pass through the isthmus and issue out of it, the characteristic gurgling sound is produced. If the stays be taken off, the sounds are no longer heard; but they may be reproduced by applying any other form of constriction to the abdomen at the same level."

Another serious class of maladies must now be considered as resulting from the use of the corset. I refer to uterine displacements. W. S. Playfair and T. Gaillard Thomas both mention tight clothing as a cause of these troubles. Doubtless many other authorities could be cited on this point had I taken the time to consult them, as the fact is so axiomatic that the person must be mentally myopic indeed who fails to see that by narrowing the waist the corset presses downward the abdominal organs, which, in turn, bear upon the uterus and produce prolapsus and anterior and posterior versions and flexions. Fothergill says: "The pressure exerted by the combined action of stays and heavy skirts upon the contents of the abdomen and pelvis is most baneful, and displacement of the womb is one of the most common consequences." In addition, the inactivity of the bowels, caused by the corset, helps to produce displacements of the womb. As Dr. Martha J. Smith has said in "The Journal of the Amer. Med. Assoc.," February 27, 1897, "Constipation is a fruitful factor in the production of general as well as local diseases, and frequently aids in the production, if it is not a direct cause, of displacements. The full bowel resting upon the body of the uterus will aid in the production of flexions and versions, and in conjunction with other causes, may produce pelvic inflammation, and it certainly aggravates whatever disease may be present in the pelvis."

Not only does the corset produce displacements of the womb, but by substituting clavicular for diaphragmatic respiration, it also renders inactive to a large extent the abdominal muscles. They, therefore, fail to develop properly. One has only to notice the small hips of our fashionable women to realize this fact. On the other hand, professional vocalists who practice abdominal breathing (I mean now first-class artistes) have very large hips, as well as broad chests. Now, a full development of the hips, the abdominal muscles, and the pelvic organs, is essential to the physiological performance of the function of generation. Thus because of the corset utero gestation is attended with more danger, and delivery becomes more difficult. It is a well-known fact that labor is becoming more and more complicated as civilization advances.

The corset is recognized as one cause of depressed nipples and mammary abscess, making suckling painful and often impossible. Both mother and child suffer in consequence. The former endures physical torture, while the latter (if weaning becomes imperative) is deprived of the nourishment provided by Nature, and its constitution is thereby enfeebled.

There is one other effect of the corset which we must consider. In doing so we must also inquire why the stay was ever invented. Why did it ever become so generally used? It is not artistic. A waist so constricted as to be out of proportion to the rest of the body, has not a single element of beauty. A statue so modeled would not be received into any art gallery. The specimens of Greek and Roman statuary that have come down to us are quite different. In them we see no wasp-like waists, but, on the contrary, the figure is well rounded and assumes the proportions indicative of health and strength. In them we see the lines of beauty and grace. They are the patterns of symmetry that have been preserved through so many centuries and will endure so long as time shall last. The figure with wasp-like waist, on the other hand, will receive from coming generations the contempt and the ridicule that it justly deserves. And why this difference? Because one is natural and the other artificial. I once read a comparison bearing on this point that impressed me very forcibly. In it the distorted waist of the American was likened to the dwarfed foot of the Chinese. What, then, is the raison d'être of the corset? The following, which appeared as an editorial in "The Medical Record" (New York) of March 23, 1895, will answer this question:

"The hand of Science falls with a dull, uncarnal thud upon the constricted waist of woman. It tells why she constricts, and that the purpose from the beginning was an unholy one. The women of Decadent Greece first began it in order to emphasize the proportions of their hips and exaggerate the delusive prominence of the bosom. The simple physiological act of respiration was perverted by the tightened girdle until the act became one of subclavicular enticement. In fine, squeezing the waist brought into lustful prominence the capacity of women for easy reproduction and subsequent plentiful lactation. Hippocrates denounced it in the women of Cos, Galen reproved the practice, Martial jeered at it, but still the waist was tightened and the double ovoid continued to glide before the ardent gaze of man. The fact, then, is that women have tightened their girdles not because they wanted to do it, but because men approved of them and desired them the more for it. Why should women, then, be blamed? The practice is admitted unsound by all authorities from Hippocrates to Dio Lewis, but men have insisted upon it. Let the sanitarist and the artist direct their attention, then to man, the brute; not to woman, his victim. When this carnal but necessary factor in society and dress reform is cured of his evil ways, women will dress as they ought, but not before."

There are, however, two sides to this question. It is all right to speak of "man, the brute," but we must not forget that woman is in this matter but too ready to yield to his desires and to dress in a way that she knows, or ought to know, will entice him. There is, then, a pernicious moral influence exerted by the corset in addition to its injurious physical effects upon the woman and her offspring. It was said by One whose teaching should be accepted by all, since it far transcends any mere code of ethics, founded, as it is, upon vital principles, that "whosoever looketh on a woman to lust after her hath committed adultery with her already in his heart." We know that the dress worn by women is frequently but too well adapted to excite man's evil passions. The stay is no exception. The history of this article shows that it was designed for this very purpose.

In conclusion, permit me to say that I have complied with the implication contained in the title of this paper; i. e., I have enumerated some, not all, of the evil effects of the corset. To give them all would tax unnecessarily your patience. Sufficient have been mentioned to show that without the use of stays and busks women would themselves enjoy better health and transmit stronger constitutions to their children, while at the same time they would avoid one method of leading men into temptation.

1232 E. Preston St.

ABSCESSES OF BOTH KIDNEYS DUE TO CHRONIC CYSTITIS.—ABSCESSES OPENED AND DRAINAGE KEPT UP.—STEADY IMPROVEMENT.*

By JACOB MICHAUX, M.D., Richmond, Va.,

Professor of Obstetrics, University College of Medicine, Richmond, Va., etc.

I desire to report a case of chronic cystitis, of five or six years' standing, resulting in abscess of the kidney, and the surgical measures resorted to for relief.

Miss C. F., white, æt. 22, was brought to me in January, 1897. She was found to be suffering with chronic cystitis, which had been present, according to her statement, and that of her aunt, for five or six years. I advised operative treatment, but could not then induce her to submit. I put her upon medical treatment, and advised rest. After the lapse of a month of fruitless effort, she consented to go to the Retreat for the Sick of this city for operation.

Accordingly, on February 18, 1897; I dilated the urethra under ether and cauterized the internal surface of the bladder. She was then put to bed, and the bladder washed out daily with a solution of teaspoonful of boric acid to a pint of tepid water. The urine drained away well, and the temperature and general condition improved fairly well for about three weeks.

About March 15 the temperature again began to climb until the 24th of March, when it reached 104° F.

For some days my attention had been attracted to the peculiar character and location of the pain, which did not seem to come, as I at first supposed, from the bladder, but was referred to the left side radiating outward and upward through the left inguinal region and over the crest of the ilium to the region of the kidney. The post-renal region was not enlarged, but was very tender. The bladder symptoms were at this period in abeyance, and yet we had fever and pus in the urine in large (visible) quantities. I, therefore, reasoned that it came from the kidney. In order to confirm the diagnosis, I sent a vial of the urine to Dr. Moses D. Hoge, Jr., Professor of Urinology and Pathology in the University College of Medicine, who, upon a careful and most complete analysis, reported renal cells among the debris.

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* Reported in part to Richmond Academy of Medicine and Surgery, May 11, 1897, and Notes up to May 25, added.

"Upon this hint I spake," and operated the following day (March 25, 1897).

After exposing the whole posterior surface of the kidney, I saw that the entire organ was enlarged and inflamed. The pelvis of the kidney did not (as I expected it would) show any signs of distention and inflammation.

Dr. J. W. Henson, Professor of Anatomy in the University College of Medicine, who assisted me, called my attention, at this juncture, to a peculiar yellowish spot near the convex border not far from the middle of the organ. On palpation, this was found to be the thin wall of a large abscess wholly within the organ. This was incised, and my index finger was carried down to the pelvis of the kidney, and found an abscess cavity about an inch and a half wide, extending the whole width of the organ, and at that point almost dividing the kidney into two parts, the capsule being about the only tissue that held the separated portions together. I was tempted to remove the whole kidney—so unpromising was the condition presented; but concluded to try to save whatever sound tissue might remain by inserting a rubber drainage tube into the kidney abscess, and dressing the wound by the open method. I put two stitches in the posterior end of the long incision and packed the rest with gauze around the tube. Drs. William T. and St. J. Oppenhimer, J. A. Hodges, J. W. Henson, William R. Jones, J. M. Winfree, also Drs. Porter and Greiner, house physicians in the Retreat, kindly assisted me.

The temperature at once fell to normal, and, with trifling variations, remained there. The general condition improved, and there was strong reason to hope for a good recovery.

About April 1, however, I was disappointed to find more temperature and bladder symptoms looming up. I combatted these as best I could by drug treatment and lavage, but without success, till April 15, when I opened the bladder between the mouths of the ureters, and inserted a glass drainage tube about five-eighths inch in diameter. This was followed by a great improvement in temperature and general condition, and greatly facilitated washing out and drainage of the viscus. I soon began to take her out of bed and make her sit in a rocker and take a few steps. [The condition of the kidneys continued to improve, save on one occasion, when I removed the tube, about two weeks after the operation upon it. Finding the temperature going up, I reinserted it, and it still remains, draining limpid urine and only small amounts of pus.] The bladder is now

(May 12) giving her more trouble than the kidney, and I fear that its walls, so long inflamed, may not be able to take on reparative action.

I shall now try continuous irrigation for three or four hours night and morning.

Just after the above report, I found that although the tube from left kidney was draining well, the one from the bladder was dry, and that there was pain and tenderness in the right inguinal and lumbar regions, clearly showing stoppage of the right ureter. I therefore, on May 14 (1897)—thirty hours after these symptoms first appeared—operated under ether upon the right kidney in order to relieve the choked ureter.

The incision was made just below (three-fourths of an inch) the lower border of the last rib. The kidney was exposed, and the pelvis and upper end of the ureter were brought into view. An incision was made at the junction of the ureter and pelvis, and the urine issued forth in a strong jet some six inches above the body of the patient. The quantity of the urine that escaped was considerable, and some clots and a little pus escaped. The depression and nausea following this operation were extreme, and I almost despaired of her life; but she rallied finally, and is now improving, though still weak. The drainage from both kidneys still continues through the rubber drainage tubes in the incisions in the back, and there is some urine from the glass drainage tubes in the base of the bladder.

I shall keep the kidneys and bladder well drained and washed out, with the hope that healing may ultimately take place, and that then the three fistulæ, which I have made, as above described, may be closed. She is free from pain now for the first time in many months, and is slowly gaining strength, but I fear for her yet.

323 East Franklin street.

SOME OVERLOOKED POINTS IN LIVER TROUBLES.*

By JACOB MICHAUX, M.D., Richmond, Va.,

Professor of Obstetrics, University College of Medicine, Richmond, etc.

The above caption opens far too wide a subject to be dealt with exhaustively, except in a systematic treatise; hence no attempt will be made to cover the whole field. My object is to give my personal experience and observations, improved by all the extraneous sources I could command in the short time at my disposal.

First, I desire to direct your attention to the confusion that exists regarding the two widely different and entirely distinct conditions known as jaundice and biliousness. A proper conception of the difference between these conditions is most important, because of the radically different treatment required for them. Furthermore, it is important on account of the aid to diagnosis of a just appreciation of the two conditions.

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* Read before Richmond Academy of Medicine and Surgery, May 25, 1897.

The occurrence of jaundice is, as a rule, only possible when the common duct is obstructed and the secreted bile is reabsorbed. (There are one or two exceptions, viz.: A rare form of blood dyscrasia and yellow fever.) Thus it appears that jaundice is the result of the absorption of bile; whilst biliousness is the muddy, dirty discoloration resulting from the non-elimination of the elements which constitute the bile. Chemistry proves that color depends upon the arrangement of atoms; hence it is easily seen that the characteristic color of the bile could not be found in the tissues before the bile is elaborated from the crude materials in the blood. Observation and experience have shown this view to be correct. Biliousness, therefore, is only the result of functional derangement of the liver, whilst jaundice is the result of obstruction of common duct (from any of several causes, such as catarrh, tumor, parasite, or gall stone, etc.). In the latter condition the liver is often perfectly normal, but is unable to deliver its secretion to the intestine.

It is readily seen from the foregoing statements that the treatment of the two conditions must be radically different. It is manifestly bad therapy to stimulate the liver in jaundice, for the reason that the organ is not at fault and an increase of the secretions would endanger the integrity of the tubes and increase the pain and pressure. Better far to give belladonna in full doses to dilate the tube by its action on unstriped muscular fiber and soda bicarbonate to render the bile more fluid, thus to facilitate its outflow; the salines to deplete and relieve pressure.

In the case of biliousness we simply stimulate the functions of the liver, and for this we have many efficient remedies. Among these are some of great value too little used, viz.: ipecac and euonymus. These, in combination with calomel, I think, are the best. I am aware of the mooted question with regard to the action of calomel; but I must confess that my faith in it as a cholagogue remains unshaken.

323 East Franklin street.

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SELECTIONS.

CHEMISTRY AND PATHOLOGY OF GOUT.

By ARTHUR P. LUFF, M.D., B.Sc., F.R.C.P.

Gout is the manifestation of a number of morbid tendencies, some of which may be inherited and some acquired, and which result in the different diseases associated with the arthritic diathesis. If the joints are affected articular or regular gout results; if other organs or tissues are affected then irregular gout is produced.

Gout is associated with the presence of an excess of uric acid in the blood, and the questions I propose to discuss in these lectures mainly resolve themselves into the mode or modes by which the uric acid is produced and introduced into the blood, the source or sources of its production, the relationship that it bears to the gouty paroxysm and to the other manifestations of gout, and the factors or conditions which influence its formation and its injurious action.

Uric acid was discovered in the urine by Scheele in 1775, and in 1787 Wollaston demonstrated its presence in gouty patients in the form of a sodium salt.

The various views held as to the primary cause of gout may be classified into the three following groups:

1. Excess of uric acid regarded as the primary cause.
2. Morbid changes in the structure of tissues regarded as the primary cause.
3. Nervous disturbance regarded as the primary cause.

I. EXCESS OF URIC ACID REGARDED AS THE PRIMARY CAUSE OF GOUT.

(1) The Uric Acid Compound Regarded as Acting Passively and Physically while in the Crystalline State.—Sir Alfred Garrod and Sir William Roberts are the two principal exponents of the view. The former regards the inflammation attending each paroxysm of gout as the mechanical effect of the deposition of sodium biurate in an articular cartilage predisposed to its reception by being the seat of former injury or disease. The deposition is determined by the blood for some reason or other becoming incapable of holding the compound in solution. Sir William Roberts also regards uric acid in solution as innocuous, harmful effects accruing only from its crystallization in the blood and tissues. He attributes irregular gout to the precipitation of sodium biurate crystals in the structures of the organs implicated, and considers that scattered needles of the salt may form the foci of gouty thromboses. Such crystals have been found by Cornil and others in the neurilemma of peripheral nerves, in the cerebral meninges, and elsewhere.

(2) The Uric Acid Compound Regarded as Acting as a Poison or Irritant while in the Dissolved State.—Many writers and observers have supported the view that, apart from the local trouble in the joints caused by the deposited sodium biurate acting as an irritant, the soluble uric acid compound which is circulating in the fluids of the body acts as a poison, the toxic effects of which are responsible for a number of the symptoms associated with the gouty state.

Pfeiffer holds that a compound of uric acid is deposited, without ill effect. in both healthy and diseased parts of the body, and that an acute attack of gout is caused by some of this being redissolved by increased alkalinity of the blood to form a concentrated solution with toxic properties. But we know that uric acid is deposited as the sodium biurate, and it will be shown later on that the solubility of this is not heightened by increased alkalinity of the solvent.

Sir William Roberts advances against the toxicity of uric acid in solution, first, the absence of experimental proof, and, secondly, the fact that on the eve of an attack of acute gout the fluids of a patient's body are saturated with sodium biurate, and yet no toxic symptoms precede the arthritic manifestations. Furthermore, in leucocythæmia and other severe anæmias the blood is frequently highly charged with uric acid, but toxic symptoms are entirely absent.

2. MORBID CHANGES IN THE STRUCTURE OF TISSUES REGARDED AS THE PRIMARY CAUSE OF GOUT.

(1) Necrotic Changes in the Affected Tissues Due to the Presence of Dissolved Urates Regarded as the Primary Cause of Gout.—Ebstein, who is the main exponent of this view, holds that a necrotic process is produced in the cartilages or other implicated tissues by uric acid in one form of combination, as the result of which uric acid in another form of combination is deposited in the necrosed areas, uratic crystals forming only in necrosed and never in healthy tissues. He holds that the neutral sodium urate in a dissolved state acts as an irritant, causing venous stasis, and eventually necrosis in the implicated tissues. The process of necrosis generates a free acid, which converts the neutral urate present in the fluids of the body into the acid urate, which is accordingly deposited in the necrosed tissues.

Ebstein bases his theory on two sets of experiments, the first of which consisted in the examination of the organs and tissues of birds which he had endeavored to render gouty by preventing the elimination of their urinary secretions; the second in observations of the irritant effect of sodium urate solution upon the cornea. The first research was carried out in two ways, (a) by ligaturing both ureters of cock, (b) by administering to them repeated small subcutaneous injections of neutral potassium chromate, which he considers to inhibit the passage of uric acid through the kidneys by its action on the renal parenchyma, both methods resulting in the formation of uratic deposits in the articulations, tendon sheaths, liver and muscles; these deposits were much larger in the latter case, in which the animals could be kept alive for a longer period. He concluded that a necrotic process had supervened upon the action of some necrotic agent (presumably uric acid) and had led to the deposition of the urate.

I do not think that the morbid processes occurring under these conditions in fowls can be considered as in any sense comparable with those occurring in connection with gout in man. Ebstein found uratic deposits in the liver and muscular tissues of the birds experimented on—localities where they are not found, at all events to any appreciable extent, in human gout. From this one may fairly conclude that the two processes cannot be considered as comparable. Moreover, since the fowl produces and eliminates by the kidneys so large a quantity of urates, the more or less sudden stoppage of kidney excretion must necessarily result in the damming back of it and the rapid accumulation of it in the blood and tissues, where, as Sir William Roberts suggests, it would probably first collect in a state of solution as the quadriurate, which would then be precipitated in the tissues as the gelatinous biurate, and this in its turn would be changed into the crystalline biurate.

The objection to this method of experimentation is that, in the first place, the solution of uric acid in sodium phosphate does not contain the neutral sodium urate, which is the body on which Ebstein relies for the production of the initial irritant effects leading on to the necrotising process. The solution would contain the sodium quadriurate, or the biurate, or a mixture of the two. Moreover, as Sir William Roberts has pointed out, such a saturated solution would soon begin to deposit its urate in the form of the gelatinous biurate, which, infiltrating the affected area of the corneal tissue, would act as a mechanical irritant. It is clear, therefore, that all the corneal changes observed by Ebstein can be accounted for by the assumption that they are caused by a mechanical irritant.

Neubauer's experiments, in which he administered enormous quantities of uric acid to rabbits without causing any apparent inconvenience, also oppose this view. Moreover, it may well be asked why, if urates are to be regarded as irritants, the kidneys, which normally excrete them constantly, ever escape damage. Furthermore, not only is there no proof that the neutral sodium urate which Ebstein regards as starting the gouty changes ever exists in the human body, but there is strong evidence that it never can so exist, as it is an extremely caustic and unstable body which is decomposed in the presence of carbonates such as those in the blood. And Pfeiffer has shown that the injection of solutions of the biurate cannot cause necrosis, a result which must apply still more to the weak solutions found in the blood. Ebstein's further assumption that the necrotic process generates an acid converting the neutral into the acid urate shows an imperfect acquaintance with the chemistry of the subject; for Sir William Roberts has shown that uric acid is primarily taken up by the blood and lymph not as a neutral urate, but as a quadriurate, and that the formation and deposition of the crystalline biurate are not favored by the intervention of an acid. Ebstein's assertion that he is able to demonstrate necrotic changes in areas from which he has dissolved out crystalline urates is not in accord with the results of other competent observers, and I myself have been unable to find in such cases any changes beyond what might be produced by mechanical pressure followed by some inflammation and degeneration.

If, too, the sodium biurate is only formed and deposited in necrosed areas, how is it that crystals of sodium biurate are occasionally found free in the synovial fluid of a gouty joint? As Sir William Roberts remarks. "it will scarcely be contended that necrotising and necrotic processes can take place in synovia."

Sir Dyce Duckworth, in a modified sense, believes that a soluble urate may act as an irritant. Although he considers that gout is primarily due to a disorder of the nervous system, he entertains the additional view that the urate in solution may set up degeneration and necrotic changes in tissues.

(2) Inflammatory or Degenerative Changes in the Affected Tissues Regarded as the Primary Cause of Gout, such Initial Changes not being Caused by Urates.—Dr. Ord in 1872 considered that gout was due to a special form of degeneration in some of the fibroid tissues, resulting in an excessive formation of sodium urate, which is then discharged into the blood, and is subsequently deposited into those parts least freely supplied with vascular and lymphatic structures. Dr. Ord, whose views in this particular have been supported by Dr. Norman Moore and Mr. Bowlby, also considers that uratic deposits only occur in tissues which have previously begun to degenerate.

3. NERVOUS DISTURBANCE REGARDED AS THE PRIMARY CAUSE OF GOUT.

Cullen, the great opponent of the humoral theory in the latter half of the last century, considered that gout mainly depended on an affection of the nervous centers. Sir Dyce Duckworth, while accepting the view as previously mentioned, that uric acid has some connection with gout, considers that gout is primarily dependent on a functional disorder of a definite tract of the nervous system, and that the part especially involved is possibly situated in the medulla oblongata, where, it may be, there is a trophic center for the joints. One reason that Sir Dyce Duckworth gives for considering this possible is the relation of gout to diabetes, the consideration of which has led him to the belief that the portions of the nervous system involved in the two diseases cannot be far apart from one another. In consequence of this disorder of the neurotrophic system defects of nutrition arise, which not only cause undue formation of uric acid, but also inhibiit the normal destruction of that body in the tissues; at the same time the renal excretory power for uric acid appears to be temporarily inhibited as part of the process of the gouty paroxysm. He draws the following distinction between inherited and acquired gout: In primary, or inherited gout the toxæmia is dependent on the inherited gouty neurosis. In secondary, or acquired gout the toxæmia arises from the digestive and excretory organs becoming overloaded, and then if with this toxæmia there is depression and exhaustion of the nervous system, the gouty neurosis may be established by the morbid blood condition affecting the nutrition of the nervous system.

Dr. Edward Liveing considers that there is much to be said in support of the view that gout is a manifestation of a disorder which has its primary seat in the nervous system.

Dr. Ralfe held the view that the accumulation of uric acid in the blood in gout was due to non-conversion into urea of the uric acid normally formed in the tissues, and he considered that the conditions which prevent the normal destruction of uric acid in the tissues depend probably on disturbance of innervation. From a careful consideration of these various views as to the primary cause of gout I am of the opinion that the greatest mass of evidence is in favor of the view that a compound of uric acid is the materies morbi.

All observers are agreed that an abnormal quantity of uric acid is found in the blood in gout. This overcharging of the blood with uric acid must be due to one or more of the following causes:

1. Normal production and deficient excretion of uric acid.
2. Over-production and normal excretion of uric acid.
3. Diminished destruction of uric acid by imperfect oxidation, or by some other means.

This last theory may be dismissed at once. There is no proof that the process of oxidation destroys uric acid; on the contrary, there is proof that uric acid is produced by a process of oxidation. Moreover, not only is there no proof that uric acid is produced during health in the organs and tissues of the body generally, and that it subsequently undergoes more or less destruction, but there are good reasons for believing that uric acid is, in health, only formed in the kidneys, and never appears in the general circulation except under pathological conditions.

With regard to the second theory of over-production and normal excretion of uric acid, there is abundant experimental proof to show that an increased production of uric acid, as in leucocythæmia and severe anæmia, does not lead to gout so long as the kidneys remain in a normal condition. In addition, gout is essentially a disease of middle age, and is only very exceptionally met with in childhood and in youth, although the formation of uric acid is greatest in early life, and apparently diminishes with the advance of age.

View that Deficient Excretion of Uric Acid is Connected with Gout. —That there is a deficient excretion of uric acid in gout is, I think, justified by the following facts. Judging as far as possible by averages, recent accurate estimations of the excretion of uric acid show that in gouty subjects there is a decrease in its daily excretion, as compared with healthy individuals.

Pfeiffer compares the quantities of uric acid contained in the urine of gouty patients of various ages, and in whom the complaint had not yet become chronic, with the quantities contained in the urines of subjects at the same age. For purposes of comparison the quantities of uric acid found by him were calculated in grammes per 100 kilogrammes of the body weight. His results are given in the following table:

Age.	Gouty Subject.	Healthy Subject.
.	Gramme.	Gramme.
30 to 40	0.885	0.965
40 to 50	0.818	0.882
50 to 60	0.701	——
60 to 70	0.661	0.752

This table indicates that the amounts of uric acid excreted by gouty subjects were always rather lower than the quantities excreted by healthy persons of the same age.

In the following table are the results of the daily determinations that I made for eight successive days, respectively, of the total uric acid excretion in the urine of three persons, namely, (a) a male patient suffering from an attack of subacute gout supervening on chronic gout; (b) a male patient suffering from chronic gout and lead poisoning, with recent pain in the right metatarso-phalangeal joint, and in both ankle joints; (c) a healthy man. The quantities of uric acid in the three cases are given in grammes, and are calculated per 100 kilogrammes of the body weight. All the individuals were between 40 and 50 years of age:

Subacute Gout.	Chronic Gout.	Healthy Subject.
Gramme.	Gramme.	Gramme.
0.260	0.578	1.105
0.263	0.617	1.027
0.315	0.665	1.020
0.350	0.715	1.376
0.442	0.443	1.175
0.556	0.372	1.030
0.506	0.593	1.252
0.494	0.594	1.203
——	——	——
Average 0.398	Average 0.572	Average 1.148

These results, I think, justify the view that deficient excretion of uric acid occurs in connection with gout. I shall have occasion in a subsequent lecture to discuss in detail the probable rôle taken by the kidneys in the production of uric acid and in the development of gout.

Admitting that an excess of uric acid in the blood, and that its deposition therefrom in cartilages and other tissues is the direct exciting cause of a gouty paroxysm, the two questions that naturally arise in the mind are: 1. Where is the uric acid formed? 2. How is the uric acid formed? I believe that uric acid is formed in connection with some diseases, notably blood diseases accompanied by leucocytosis, in a different manner to that in which it is produced in ill health and also in different organs. I also hold the opinion that the source of the uric acid contained in the blood in gout is the same as that from which the uric acid eliminated in the urine in health is derived. I propose, therefore, first to consider the various views as to the seat or seats of formation of uric acid in health and in gout.

SEAT OR SEATS OF FORMATION OF URIC ACID IN HEALTH AND IN GOUT.

View that the Kidneys Form as well as Excrete Uric Acid.—This theory is maïntained by Sir Alfred Garrod, who concludes that uric acid in the blood, if not absorbed from the stomach, must arise from that formed in and by the kidneys, the blood changing the salt from the ammonium to the sodium urate. Kolisch also regards the kidneys as the most important of the uric-acid-forming organs. Dr. Latham considers that the kidney is the seat of the final formation of uric acid by the conjugation of substances manufactured in the blood. Zalesky compared the effects of extirpating the kidneys and of ligaturing the ureters in serpents, which excrete almost pure uric acid; the animals lived about the same time in either case, but in the latter only were uratic deposits found in the tissues. This he takes to confirm the above theory, which is supported also by the observation of Garrod and others that the administration of uric acid or its injection into the blood caused an increase of urea in the urine, but not of uric acid.

View that the Liver and Spleen Produce Uric Acid.—The view that the liver is the main manufactory of uric acid probably depends on the fact that the excretion of this substance is greatest during digestion; this fact is, however, compatible with the idea that the liver forms only the precursors of the acid which are combined in the kidney. Schroeder states that uric acid is normally found in the blood of birds, and that it continues to be formed and, indeed, accumulates in the liver and blood after the removal of the kidneys. This is directly opposed to Zalesky's experiments; further, if uric acid is formed in the liver and conveyed to the kidneys, it must be present in the blood, and I shall shortly show that the blood of birds is entirely free from uric acid. Minkowski succeeded in keeping geese alive from six to twenty hours after extirpation of the liver; after the operation, their urinary excrement contained only 2 or 3 per cent. of uric acid, instead of the normal 60 or 70 per cent. These results are, however, equally compatible with the view that the liver is the seat of production of the antecedents of uric acid.

Dr. Murchison regarded the liver as the seat of production of uric acid, and the presence of the latter in the blood or tissues as the result of functional derangement of the liver, as the result of which the metabolism of some of the albumen is arrested at the stage of uric acid formation, instead of going on to the complete stage of urea formation. Charcot regarded the liver as the principal seat of production of uric acid. He considered that a functional derangement of the liver caused the production of excessive quantities of uric acid, and its consequent accumulation in the blood. Meissner regards the liver of fowls in the normal condition as the principal source of uric acid, but considers that the spleen and the nervous tissues share in the formation. Ranke was of opinion that his experiments led to the conclusion that the spleen was the principal organ concerned in the production of uric acid. It has, however, never been possible to show that the spleen takes any active part in developing the gout.

Views that Uric Acid is Produced in Various Tissues.—Ebstein, who attributes in cases of gout the main production of uric acid to the muscles and bone marrows of the affected extremities, admits, however, that the kidneys may take a part, not only in the secretion, but also in the manufacture, of uric acid. Robin was the first to suggest that uric acid was normally formed in all the tissues, and that the condition in gout was only an exaggeration of the physiological one. Chrzonsczewski, Cantani, and Senator also considered that uric acid is formed in cartilage and fibrous tissue, but most observers agree that it is only deposited, and does not originate in tissues of the connective group. Dr. Haig asserts that in addition to the formation of uric acid in the animal body, the amount present depends greatly on that taken in. He does not, however, produce any proof of the storage of uric acid in the system apart from gout, and it must further be remembered that in disease such as leucocythæmia, in which large quantities of uric acid are formed, they are not stored, but eliminated.

Does Uric Acid Occur Normally in the Blood of Man and other Animals?—The solution of this question is of great importance in relation to the problem as to whether uric acid is formed in or carried to the kidneys. I am able to confirm the conclusions of Garrod, von Jaksch, and Klemperer that uric acid is absent from the blood of healthy persons. Of other mammals I have examined the blood of the ox and sheep, and have found no trace of uric acid in them, a result which is again in accordance with those of Sir Alfred Garrod. In the blood of the turkey, goose, duck, and fowl I found, like him, no uric acid, but plenty of urea. Sir John Davy also failed to find uric acid in the blood of snakes.

This mass of experimental evidence shows that uric acid is never present in the blood of birds and serpents, although their urinary excretion is almost entirely composed of a compound of uric acid, conclusively supports the view advanced by Sir Alfred Garrod, and held by Dr. Latham, and in a modified sense by Kolisch, that uric acid is normally produced in the kidneys. How is it possible in all these cases that uric acid could be absent from the blood if the view be correct that uric acid is formed in the system generally, and is conveyed in the blood to the kidneys, which play, as it were, merely the part of a filter in the removal of uric acid from the blood? I consider that the evidence brought forward renders such a view impossible.—Br. Medical Journal.

SYPHILIS AND SEXUAL NEURASTHENIA: THEIR TREATMENT WITH THE GOLD SOLUTIONS.

By HERMAN F. NORDEMAN, M.D.,

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Within the past six months my attention has been repeatedly called to the gold solutions (known as arsenauro and mercauro) as offering results in my special line of practice not heretofore obtained by treatment commonly in vogue. I am skeptical in regard to special therapeutic agents, as a rule such claims being made by the manufacturer as would tend to preclude confidence rather than produce it.

Knowing the reputation of G. Frank Lydston, M. D., whose writings are so authentic and forcible, and being attracted by his paper read before the Chicago Academy of Medicine, in October, 1894, I determined to test these alteratives thoroughly in my service at the New York Polyclinic, and thus learn from personal observation whether results obtained would warrant me in suggesting their use to the number of practitioners who attend this institution. I must say that the outcome has been beyond all expectations.

The classes of cases that came under my observation were as follows: Chronic masturbators, those suffering from the results of incomplete coitus, or certain degenerates, the victims of psychical excitants leading to complete or partial impotence. These are the milder classes of cases so ably described by Kraft-Ebing in his classical work on pyschopathia sexualis, and by Schrenk-Notzing in his Suggestive Therapeutics. These patients present themselves with the usual symptoms, i. e., profound anæmia, neurasthenia, relaxed scrotum, etc., etc. In other words, they belong to the list of sexual neurasthenics. They feel the desire for copulation, but the act is unsatisfactory in that the moment the organ becomes erect, ejaculation takes place, rendering the sexual congress incomplete and disgusting. In several cases I began the administration of arsenauro in five drop doses, increasing daily until the patient reached ten drops, three times daily, continuing this dose for at least eight weeks. It has been interesting to note the excellent general appearance of these individuals after about two weeks of treatment. They were full blooded, hearty and buoyant, differing so markedly from their appearance when they presented themselves—an appearance so familiar, so unmistakable to medical men. I think without doubt that arsenauro is the most pronounced aphrodisiac I have ever seen, producing this effect in both sexes. Thus, after noting the effect, I was led to extend its use in my private practice.

The nervous debility so marked in the sexual neurasthenic is positively controlled by arsenauro. As for mercauro, I am in a position to attest its great value in cases requiring tonic mercurial treatment. I refer to those instances where, in the initial lesion, we give either the protiodide of mercury or inunctions of the ointment. Our patients often rebel at a continuation of this treatment. It is here that I put the patient on mercauro, beginning with five drop doses and increasing a drop a day until evidences of arsenical intolerance are established. Look for puffiness under the lids which may appear when arriving at eight, ten, fifteen or twenty drops, three times daily, or the intolerance of the gold may be presented—i. e., frontal headache, a tendency to vertigo, and increased saliva. When any of these symptoms are present, decrease the dose until they disappear, then resume and persist in the treatment—say for at least six weeks without omission. I find these patients who are run down, as it were, under so-called tonic mercurial treatment, rapidly build up and show such an improvement that I continue them right along with mercauro and apparently abort the later specific lesion—i. e., locomoter-ataxia, hemiplegia, etc. In mild forms of syphilis with little or no glandular involvement, and in mixed sores, I immediately resort to mercauro, and I have never seen its equal as a remedial agent in syphilis. It is pre-eminently a new therapeutic agent, the physiological effect being subjudice. In latent lesions it is positively the best remedy. By abundant experience I am settled in my conviction as to its value, and more particularly so, when the iodides are not tolerated. We meet many cases that present an idosyncrasy, either with little or much of the iodide of potash. In these cases, I give mercauro at once, and I am satisfied that my colleagues will agree with me as to its value.

Squamous, ulcerative and tubercular syphilides, hyperplasia and chronic mucous ulcerations are the special indications for mercauro. In lesions of the skin, such as a tendency to eczema where syphilis coexists, this is a most powerful antidote therapeutically. In chronic catarrhal cystitis, as the result of prostatic hyperplasia, arsenauro is a very valuable remedy. I have seen cases which have existed for years where the bladder would only contract sufficiently to expel a portion of the urine, leaving a residue to undergo ammoniacal decomposition, become absolutely well under the use of this combination. Seemingly it stimulates the viscus to contract, and I have noted the same result in the hypertrophied prostate of the aged, where atony of the bladder existed. It has been remarkable to note the vigor this solution gives to the organ. Of course mercauro must be given preference where a history of specific disease exists, or is suspected. I feel under obligations to those of my colleagues who attracted my attention to these products, and I hope that some one among my hearers at the Polyclinic will in time work out their physiological effect. They are certainly valuable curative agents and deserve a careful trial. As illustrataive, I mention an interesting case. Mr. R. J., age thirty-two, widower, sent to me by Dr. L. to be examined for diabetes. He had at times, as the doctor informed me, shown traces of sugar in his urine. The patient was told by Dr. L. and other physicians in New York that his malady was diabetes—the result being that he came to me in a state of extreme mental perturbation, almost verging on nervous prostration.

Examination of urine revealed $\frac{1}{4}$ to $\frac{1}{2}$ of one per cent. of sugar, high specific gravity and hyperacidity. His family history was good, both parents alive and brothers and sisters healthy. He complained of violent headache, loss of sleep; in fact, he was in a profoundly anæmic condition, and suffered from neuralgias in his arm, shoulders and lower extremities. Examination revealed hyperæsthesia of the skin, tenderness along the lumbar spine, and last, but not least, a well-marked specific macular eruption. Recently coitus was denied absolutely. Upon examining the throat mucous patches were discovered on the soft palate and a well-defined chancrous sore on the right tonsil.

The cervical glands were markedly enlarged. I made the diagnosis of syphilis, as the case was clearly this, and the subsequent treatment proved it beyond any doubt.

The patient was placed on a full diet, allowed some claret, as he was sorely in need of a general tonic, and mercauro prescribed in ten drop doses three times daily, increasing one drop every other day until he was taking twenty drops three times daily, which was his physiological limit. His general condition began to improve in a very short time—sugar disappeared entirely, the eruption and sores improved, headache and nervousness left him, and he gained in flesh and strength. Here then was a typical case of the beginning secondary stage of syphilis benefited by mercauro, the case being one in which the use of other well known mercurials could not have brought such a result. I mention this case to show the good effect this mercurial tonic will produce, especially in syphilitic anæmias.—The N. Y. Polyclinic.

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ABSTRACTS.

IN A LETTER FROM VIENNA TO THE VIRGINIA MEDICAL SEMI-MONTHLY, DR. JAMES N. ELLIS GIVES THE FOLLOWING EXCELLENT RESUME OF PROF. ROBERT KOCH'S LATEST DISCOVERIES.

Dear Doctor.—I have before me a recent edition of the "Deutschen Medicinischen Wochenschrift," containing the promised contribution from Dr. Robert Koch about his "New Tuberculin." Although his publications some six years ago generated hopes that have since proved to a great extent fallacious, one cannot but look toward the later utterances of this renowned bacteriologist with an interest born of his undisputed reputation as the acknowledged scientific authority on such subjects; and I feel that you and your readers will be interested in what he has to say.

In the present paper, he states that he has uninterruptedly continued his experiments in the employment of cultures of tubercle bacilli for the treatment of tuberculosis, and that he has now arrived at sufficiently definite conclusions to justify a publication of the results obtained. "The finished production of this work," he says, "continued for years, will seem simple and, in many respects, self-evident; but any one who has worked about bacteriological laboratories knows how troublesome and time-taking these experiments are, and what a trial they are to one's endurance and patience. And I take this opportunity to gratefully acknowledge the indefatigable and faithful assistance of Dr. M. Beck.

"The employment of bacteria and their products for curative and preventive purposes is, in the end, always a matter of immunization; and I believe it desirable to give, briefly and precisely, my position as to the questions involved, to prevent being subsequently misunderstood.

"The immunity against infective diseases was originally held to be something comparatively simple, but at length we have come to understand that it may be very complex. It will be best to illustrate by some examples. Behring and Kitasato have shown that animals can be immunized against tetanus. For this purpose, one uses the infiltrated liquids of cultures—i. e., the products of tetanus bacilli soluble in water, in which is found the specific tetanus bacilli. Against this poison, or rather against very great doses of it, the immunized animals are protected, but only for a limited time. The immunization has no influence on the tetanus bacilli themselves, which vegetate in the immunized body as much as their anærobiotic growth permits, without hindrance. We deal here with a simple and pure ptomaine immunization, and, it is possible, that if this artificial immunity has disappeared after a few weeks, the always further produced tetanus poison regains the virulence of which it has been temporarily deprived, and the animal dies of tetanus without having been reinfected by tetanus bacilli.

"The opposite condition is found in cholera and typhus. The experiments of Pfeiffer with these diseases have proved that the animals immunized by fresh agar-cultures are protected against the living bacteria, but not against the poison produced by these bacteria. The living cholera and typhus bacteria perish in the immunized animal's body in a surprisingly short time, but we have not succeeded in immunizing animals above a certain dose of poison. In this case, the immunity is a purely bacterial one—i. e., one directed exclusively against the bodies of the bacteria.

"The ideal immunization will always be one that protects the animals or the human body, not only against one of the many injuries incident to pathogenic organisms, but against all of them. These considerations play an important rôle in tuberculosis. Upon first thought, it appears that surely there can be no immunity against tuberculosis, as it can last for years in the human body without diminishing man's receptivity for it. Even in those cases where healing takes place, the man is not protected against a new invasion of tubercle bacilli. Experience teaches that he has, on the contrary, become more susceptible to fresh tuberculous infection. And yet, there are indications that tuberculosis also, under certain conditions, admits of a kind of immunization; indications that are derived from observations which one can make of miliary tuberculosis in man and of experimental tuberculosis in the guinea-pig, where generally a time comes when the tubercle bacilli (which were visible in great numbers in the earlier stages) disappear so completely that one is compelled to look very carefully in order to detect any remaining. This is very striking, as we will see further on that the tubercle bacilli, when experimentally injected into the body, are absorbed very slowly. It seems that in these cases we have an immunizing process that is a purely bacterial one. Unluckily, it comes too late to be of any use. But these observations have been sufficient reason for me to seek persistently after a method which would effect a state of immunity in an earlier stage of tuberculosis, when it would be of material use.

"The two processes above-mentioned have one thing in common, viz.: the body is overpowered in a short time by tubercle bacilli. This circumstance seems to me to be the explanation of the fact that a pronounced immunity is not obtained under ordinary conditions. The usual course is that the tubercle bacilli grow slowly and only in a small number of the organs or tissues of the human body. They generally find themselves surrounded and cut off by necrotic tissue, and it is only a long time after they have died, and after they have probably undergone chemical changes, that they are absorbed. . Where the tubercle bacilli grow luxuriantly (for instance, in caverns and on the surfaces of mucous membranes), they are unchangedly thrown off and not absorbed, and no immunization can take place under such conditions. This immunization only occurs when numerous tubercle bacilli, as with miliary and guinea-pig tuberculosis, spread quickly through the whole body and enter into interchanging effect with the living tissues.

"In the effort to obtain an artificial immunity conditions had to be striven for which should, as nearly as possible, coincide with those above indicated. But, at once, insurmountable difficulties opposed themselves. All efforts to make absorbable the unchanged living, or even dead, tubercle bacilli, in sufficiently large quantities, from the subcutaneous tissues of the abdominal cavity or from the blood vessels failed, and other experimenters have succeeded as little as I. If subcutaneously injected, the dead tubercle bacilli invariably cause pus, and they can be found for months, in the abscesses thus caused, in great numbers and capable of being colored. If injected into the abdominal cavity of experimental animals, they are a little better absorbed, and I have succeeded in effecting distinct immunity in this way. But it invariably caused circumscribed inflammations with their consequences, such as adhesions of abdominal organs, intestinal obstructions, etc., from which a large per cent. of the animals died. Dead tubercle bacilli, injected into the circulation of trial animals, caused the same tubercular nodules in the lungs that the live ones did; and in these nodules for a long time one finds unchanged tubercle bacilli. Therefore, I concluded that the absorption did not proceed in a desirable way.

"After thus satisfying myself that the tubercle bacilli, in an unchanged state were useless for purposes of immunization, I tried to make them absorbable by chemical alteration. For this purpose, the bacilli were treated with a solution of mineral acids, or with strong alkalies at boiling heat. In this manner, I succeeded, it is true, in so altering the tubercle bacilli that they were absorbed, in sufficiently large quantities, entirely from the subcutaneous tissue. But evidences of immunity did not occur, and it is to be supposed that this chemical action effected such a radical change on the bacilli as to entirely destroy its immunizing quality. I next tried an extract of the absorbable portion of the bacilli, rejecting the solid mass. First, I tried a glycerine extract, which led to the discovery and subsequent use of 'tuberculine.'

"Tuberculine has the highly valuable quality of causing a characteristic reaction when subcutaneously injected into man or animals sick of tuberculosis. This quality renders it of practical use in the diagnosis of tuberculosis in its earliest stages; a stage when its recognition by physical examination is impossible, but when there is most hope of therapeutic success. The employment of this agent as a diagnostic auxiliary, which I emphatically recommended in my first publication about tuberculine, has been more and more substantiated by time. It is employed to-day in most of the civilized world for the early diagnosis of cattle tuberculosis. The organized fight against this widely disseminated disease is the outcome of the recognition and use of tuberculine, and has already been crowned by great success. The many thousand injections that have been made on cattle for this purpose have proved how unfounded was the fear that, in consequence of the reaction, the tubercle bacilli would be made mobile and would be sent to healthy parts of the body.

"This agrees entirely with my own experience in its employment in more than a thousand cases for the early diagnosis of tuberculosis in man, of which Dr. Nietner will make a detailed report. In these cases, there was never the slightest reason for supposing that the tubercle bacilli were made mobile.

"In view of these facts, one should finally abandon the foolish prejudice of the tubercle bacilli being made mobile, and should utilize the diagnostic power of tuberculine to defeat the disease in man, as is being done in the struggle against cattle tuberculosis. In this connection, I can only repeat what I have said at an earlier opportunity, that in tuberculosis prophylaxis is infinitely more important and advantageous than all therapy.

"The further usefulness of tuberculine consists in its employment in the treatment of tuberculosis, as after each tuberculine reaction an unmistakable amelioration of the tuberculous process is observable. It lay very near me to continue the reaction as long as amelioration was called forth, but this cannot be done without limit. Slowly the capability of reaction disappears, and at the same time, of course, the effect of the tuberculine;

until finally, there is a complete immunization against tuberculine, which may last for several months. This immunization has no influence upon the tubercle bacilli themselves. It is a pure ptomaine immunity, but not a bacterial one. Unfortunately, the capability of reaction from tuberculine disappears before a cure has taken place. But the return of the disease is to be again treated with tuberculine, as soon as capability of reaction recurs. In this way, one who possesses the necessary skill and persistence can effect considerable amelioration in a great many, if not in all, cases of uncomplicated tuberculosis. Believing this, I should have continued to think tuberculine the best of all agents at our command against tuberculosis, if I had not more recently succeeded in making a preparation from cultures of tubercle bacilli possessing real immunizing power against bacteria.

"Among the many experiments made with a view to obtaining a bacterial immunizing substance, which were afterwards discarded, there is one I should like to mention which occupied me for more than a year, and which is very instructive. The preparation was an extract of tubercle bacilli, obtained by 10 per cent. normal natron-langes. The tubercle bacilli were well mixed in the liquid, left for three days at the temperature of the room.and occasionally agitated. Then the liquid standing over the culture masses was filtrated through blotting paper and finally neutralized. The liquid thus obtained was clear and slightly yellowish, and was not entirely free from tubercle bacilli. It contained sufficient of them for from five to ten bacilli to be seen on the field of the microscope in an ordinary covered-glass preparation. But they lay always single, never in masses, indicating that they were certainly dead, which I had already ascertained from previous experiments, finding that they invariably died when subjected to a 10 per cent. solution of normal natron-lange for from twelve to fifteen hours. This preparation, being an alkaline extraction, for the sake of brevity was designated "T. A." (Tuberculine Alkaline). Experiments made with T. A. proved that, when given in very small doses, it caused reactions identical with those of tuberculine, only the reactions were of a little longer duration and the capability of reaction extended over a longer period of time. But the chief thing observed was that the effects obtained from it proved to be more constant and permanent than those caused by tuberculin. Relapses were less frequent and longer deferred. But with this preparation was an injurious effect that forced me finally to abandon it. Upon reaching certain (it is true they were pretty large) doses abscesses formed at the point of injection—abscesses which were perfectly sterile, and which could only have been caused by the dead bacilli contained in the preparation. From this time on the liquid was filtered through clay cells, and thus the tubercle bacilli were entirely removed from it.

"That at certain doses of T. A. abscesses invariably occur, I believe to be a significant and an important fact, viz.: That we cannot expect the patient to accustom himself to immunization against the tubercle bacilli prescribed in their original form and injected subcutaneously. In the beginning, I injected the smallest doses of this preparation, which could have contained very few tubercle bacilli, and they were well absorbed. Then I gradually increased the dose until, after twenty or thirty injections, the larger doses were reached and abscesses were caused. If it were possible for the human body to become tolerant of greater quantities of tubercle bacilli, it ought to have here occurred. But this did not happen, clearly because the subcutaneous tissue is able to cope only with a very small number of dead tubercle bacilli. If one should wish to introduce a large quantity of tubercle bacilli in this way, one must make a number of injections in different portions of the body, which is clearly impracticable.

"These experiences with T. A. suggested the idea of destroying the tubercle bacilli mechanically, as they were by no means satisfactorily absorbed when intact; and, if possible, to disintegrate them so completely that they could be easily grasped by the absorbing elements of the body. In this idea I was sustained by observations which I had made with reference to the chemical conduct of tubercle bacilli. At the before-mentioned experiments to reduce the tubercle bacilli to a soluble condition by mineral acids and strong alkalies, I found that the bacilli contained two characteristic chemical bodies belonging to the unsatiated fat acids. One of these fat acids is soluble in weak alcohol and easily saponified by natron-lange. The other is soluble only in boiling absolute alcohol or ether, and is very difficult of saponification. Both accept the so-called tubercle coloring; that is, are dyed intensely red by carbol-fuchsin, and retain this color after being subjected to a weak saltpetre acid and alcohol. As the first of the two fat acids is soluble in alcohol, it is easily extracted, so that at the process of coloring employed subsequently on the tubercle bacilli, only the second fat acid, insoluble in cold alcohol, remains. This fat acid fixes the coloring material, and is therefore the bearer of real coloring of the tubercle bacilli. By using hot natron-lange, one can drive the fat acid slowly out of the tubercle bacilli, and, with the assistance of the microscope, one can easily follow the process of separation, and watch the fat acid as it passes out from the bacillus in the form of little colored drops that flow together, forming large drops. Meanwhile, the bacilli keep their original shape, but no longer retain the characteristic coloring of the tubercle bacilli, but only that of the other bacteria. These fat acids form, as the microscopic image of the colored bacilli teaches, a continuous layer in the body of the bacillus. They protect it against exterior violence, and explain the difficulty attending its absorption. The point, therefore, is to destroy this protecting envelope if the tubercle bacilli is to be made absorbable.

"My first attempt in this direction failed. Every effort, by pressure and grinding, to reduce the mass to powder form left the tubercle bacilli intact. It was only when I took well dried cultures and worked on them, without mixing, in an agate mortar with an agate pestle for a long time that I could observe that the dyed tubercle bacilli diminished in number, until finally only a few bacilli remained intact. In order to get rid of these, I mixed the mass thus pulverized with distilled water and subjected it to a very strong centrifugal motion. (Four thousand revolutions to the minute, for from one-half to three-quarters of an hour.) The mixture then separated into a perfectly clear and transparent upper layer, which was whitely opalescent and which contained no bacilli; and into a tenacious, slimy residuum, which was dried again, then worked in the mortar and again centrifugated as before. It yielded then equal quantities of a clear upper layer, and a thick residuum. This manipulation could be continued until finally the residuum consisted only of impurities, such as cotton fiber, duct, etc., which had been originally in the culture or had afterwards entered by chance. In this way the whole mass of tubercle bacilli was easily changed into a perfectly clear liquid. This experiment, which everybody can repeat without difficulty, was the starting point for my subsequent studies.

"Next, I convinced myself, by experiments on animals and afterwards on man, that the preparations obtained in this way were perfectly absorbable and never caused abscesses, provided they were well contrifugated and no longer contained colorable tubercle bacilli. I proved that the first obtained liquid (the upper, clear transparent layer) was the one which differed materially from the others, which are practically identical. I have designated this upper layer, obtained after the first centrifugating, 'tuberculin O.' (abbreviated to 'T. O.'), while the liquid obtained by working out the remnants left after the first centrifugating, I have called 'T. R.' A microscopic examination of the residuum after the first centrifugating, shows that the bodies of the tubercle bacilli are not changed into a uniform substance. Upon subjecting it to carbol-fuchsin or methyl-blue, one no longer sees any red-colored masses or little drops of fat acid, but in your microscopic field there are palely-colored, cloudy formations which are seamed with little dark blue drops that heap themselves especially toward the edge. After the separation into the liquids T. O. and T. R., one finds the violet cloud-like formations in T. R.; the blue-colored masses in T. O. Another difference consists in the fact that the addition of 20 per cent. of glycerine does not change T. O., but produces a fleakywhite precipitate in T. R., over which remains a perfectly clear liquid. I shall report on another occasion other interesting chemical differences.

"Their conduct with reference to glycerine goes to show that T. R. contains especially those parts of the tubercle bacilli which are insoluble in glycerine, and therefore are incapable of extraction by glycerine; while T. O. contains the parts soluble in glycerine. With this supposition, agrees the experiments made with the two preparations on animals and man. T. O. stands very close, in its qualities, to the ordinary tuberculin. Its effects coincide nearly entirely with those of the T. A. before mentioned, except that with T. O. no formation of abscesses is to be feared, but it also has but little immunizing power. T. R., on the contrary, effects decided immunity; it also causes reactions in tuberculous patients if too large doses are employed, but its power is entirely independent of these reactions, while in using the ordinary tuberculin and T. A. and T. O., reactions must be called forth in order to inaugurate a healing process. In employing T. R., I try to avoid reaction and endeavor as quickly as possible (though exercising all possible care), by gradually increasing the dose, to make the patient tolerant of large doses of the remedy; that is, to immunize him against T. R., and thereby, as I believe I have the right to infer, against the tubercle bacilli themselves.

"That T. R. contains all that is of immunizing value in the cultures of tubercle bacilli, is evidenced by the fact that a man who is immunized by T. R. reacts no more upon great doses of the ordinary tuberculin; which is proof conclusive that T. R. immunizes against all component parts of the tubercle bacillus. I state this effect of T. R. because it seems to me very important, in such a great number of cases, that no doubt should exist about the correctness of this observation.

"If T. R. is to exhibit its full effectiveness, some unavoidable conditions must be fulfilled in its manufacture. First of all, not every tubercle culture is suited for the preparation of an efficient T. R. By experiments which I have recently made, with the assistance of Dr. Vagedes, and which will be published later, I have convinced myself that tubercle bacilli are not as uniformly virulent as we have generally believed. Those cultures that are far removed, by having passed through successive culture liquids, have, as a rule, lost much of their virulence. One should employ highly virulent cultures for the preparation of T. R. The little virulent cultures furnish little effective, or even entirely ineffective, preparations. They must not be too old, and they must be worked as quickly as possible. The drying must be done in the vacuum-exsiccator, as other manipulations easily work such changes that the substance becomes less soluble, and, at the same time, less effective. Especially are all chemical manipulations, however small, to be avoided as much as possible, as T. R.

seems to be peculiarly sensitive to them. For the same reason the cultures which are to be worked out, as well as the prepared liquids, are to be carefully protected from the light. The cultures must be worked as fresh as possible—i. e., immediately after they have become perfectly dry, as any delay injures them for this purpose. Not more than about one hundred milligrammes can be worked with the hand, and great practice and skill are necessary to deal successfully with even this quantity. A sufficient seperation of T. R. and T. O. has only been effected when the perfectly clear T. O. has taken up at least 50 per cent. of the solid substance. Otherwise, a more or less quantity of T. O. remains in T. R., and one gets undesirable reactions after injecting. He who wishes to work at the making of T. R., must remember that highly virulent, living tubercle bacilli in a dry state are to be manipulated in such a way that the formation of dust cannot be avoided. I believe the danger incident to it is no small one, and I must admit that I felt, at these experiments, as if I had to do with explosive materials. I have not used a respirator, because it did not seem to offer sufficient protection. It could not prevent the dust of the bacilli from depositing itself on my clothes and flying about the room. For these reasons, I do not believe it possible to manufacture T. R. in large quantities by hand. Only large manufactories, with the help of especially adapted machinery, can produce a uniformly good preparation in quantities sufficient for all demands, and successfully avoid the dangers incident to its preparation. The color works of Messrs. Lucius and Bruning, at Hachst-on-the-Main, have decided, on my suggestion, to make the machinery necessary for manufacturing it on a large scale, and they will sell the preparations T. R. and T. O. prepared exactly after my directions.

For the purpose of preserving the liquids, 20 per cent. of glycerine is added, which, according to my experience, sufficiently protects them against putrefaction without impairing the solubility of T. R.

"The method of determining the proper dose of the preparation is a very simple one. The injections are made, as with tuberculin, with a well-sterilized syringe into the subcutaneous tissue of the back. The liquid contains, in a cubic centimeter, ten milligrammes of solid substance, and in employing it is to be diluted with normal saline solution (not with phenol solution). One begins with .05 milligrammes, and this is such a small dose that it is exceptional for a reaction to take place. If this should occur, you must further dilute it. The injections are made every second day with such a gradual increase of the quantity that a rise of temperature beyond half a degree centigrade is to be avoided if possible. If there is any greater rise of temperature caused by the injections, they must be discontinued until it entirely disappears. I have, as a rule, increased the dose until twenty milligrammes was reached, and then, if no reaction occurred, have discontinued them entirely, or only injected after a considerable interval.

"If one would immunize animals one gives from the first as much as they can well absorb. For instance, from two to three milligrammes to the guinea-pig, and in proportion to larger animals. With tuberculous animals one must, of course, begin with much smaller doses, since doses of two milligrammes can be fatal under such circumstances.

"I will restrict myself here to some brief remarks as to the effects to be had from T. R., contributing a report about this later, when I will especially go more into detail about the treatment of patients. In the immunizing of healthy animals and the treatment of sick ones all depends on subjecting them to as big a dose as possible. Of course, they must be so measured as to insure perfect absorption, a matter which is easily determined by the conduct of the places at the point of the injection. By observing these conditions I have succeeded in immunizing a large number of guinea-pigs so perfectly that they supported inoculations with virulent cultures without being infected. The places at which the inoculation was made disappeared without a scar, and the inguinal glands adjoining were in many cases, for months after the inoculation, entirely unchanged; in others they became slightly enlarged, but without tubercular involvement. We could find no tubercle bacilli in them.

"In a number of animals immunization was not completely effected at the first inoculation. In such cases I found the inoculation wound healed, but the inguinal glands were cheesy. The internal organs, on the contrary, were free from tuberculosis. At the same time, the control animals showed far advanced tuberculosis of lungs, spleen and liver. I believe that a still lower degree of immunity existed at the time of inoculation in cases where only the lungs of the trial animals provéd to be tuberculously affected. Meanwhile, liver and spleen exhibited only traces of tuberculosis or were entirely free from it. This condition, except in these cases, I have only seen with tubercular guinea-pigs which were treated with the ordinary tuberculine, but in a too advanced stage. With guinea-pigs which had received injections of T. R. it often happened that tuberculous infections occurred, and without exception a more or less retrogressive change was noticed in the organs already tuberculous at the commencement of the treatment. This was seen especially in liver and spleen. In the liver there were no necrotic, yellowish colored foci; instead, one saw furrows and depressions on the surface, which gave a characteristic nodulated appearance to the organ. The spleen, at the same time, showed shrinkage to such an extent that in some cases only a very small portion of the organ remained, and which could only be found after careful search.

My impression derived from these experiments, is that the immunization takes place about two or three weeks after reaching the maximum dose. Therefore a cure of tuberculous guinea-pigs, with whom this disease pursues a rapid course, succeeds only when the treatment is begun early, say from one to two weeks after the inoculation. This fact is also important for the tuberculous man, whose treatment should not be begun too late. In the beginning such small doses are given that no considerable immunization is to be expected, but when one arrives at doses from one-half to one milligramme unmistakable signs of immunization are observed. Therefore, the preparation has a limited usefulness. A man whose condition is such that only a few months of life remain will not be benefited by it. No more will one who suffers from secondary infections, especially such as are complicated by streptococci and in whom septic processes have pushed tuberculosis into the background.

"In an immunization against tuberculosis complicated by streptococci and other pathogenic organisms, which so often play such a fatal rôle in advanced tuberculosis, the influence of tuberculine, at best, cannot be immediate. Such conditions are, as a rule, to be recognized by the temperature, etc.—and experience teaches that patients whose temperature exceeds 38° C. are only exceptionally receptive for the specific treatment of tuberculosis.

"The new preparation has been employed by me on a large number of appropriate patients, and, without exception, I have obtained considerable amelioration; much greater than the success obtained from the ordinary tuberculine or T. A.

"I purposely speak only of amelioration, though, after ordinary ideas, not a few cases could be designated as cured, but I believe it premature to employ the word 'cured' before a sufficiently long time has elapsed. With lupus patients was it especially noticeable that the local reactions were very slight, and in spite of them a considerable progressing improvement took place. Likewise with the phthisis there were no reactions, where, with tuberculine, there were always temporary infiltrations in the lung and stormy reactions. As a rule, in using T. R., the single local symptom was a slight increase of the crepitant râles which, however, soon disappeared. After a few injections the quantity of the sputum diminished and finally ceased, wherewith, of course, the finding of tubercle bacilli disappeared. The crepitant râles now disappeared from the diseased parts of the lungs, and the area of dulness diminished. In no case have I seen any alarming complications or other injuries to health that could be ascribed to the preparation. Nearly all patients increased in weight from the beginning, and by the end of the treatment attained to a considerable increase. Especially noticeable was the change in the temperature curves. With patients who had the well-known daily fluctuations of temperature of one or more degrees centigrades, it was noticed that the angles on the chart disappeared more and more, and gradually approximated to the nearly straight, normal line directly under 37° C.

"If my present method of injecting subcutaneously the smallest doses of T. R., and gradually increasing until twenty (20) milligrammes are reached, is the best I dare not assert, as it is possible that other methods are better and will lead more quickly to the desired end. Perhaps combinations with T. O., or with other serum preparations which have been derived from T. O. or T. R., may prove more satisfactory. Further experience must determine this; but, I am convinced that, without doubt, further improvement of these preparations themselves are not to be expected. They consist of highly virulent fresh cultures, which have been living just before, and which have been converted into a soluble condition without chemical alteration. It is not possible to make anything better in this respect, and what it is possible to accomplish with tubercle culture can be done alone with these preparations."

ABSTRACT OF PAPER ON THE TREATMENT OF COLLES' FRACTURE.*

By EDWARD A. TRACY, M.D., Boston.

In 1814, Colles, of Dublin, published his observations on the fracture which to-day deservedly bears his name. Previous to that time, this injury practically masqueraded under the description of backward dislocation of the wrist, or forward dislocation of the lower end of the radius! for such dislocations were considered common by the masters from Hippocrates down. The nearness of the fracture to the wrist-joint, the peculiarities of its anatomical surroundings, the absence of the characteristic sign of fracture unless specially sought for, and finally the teachings of the masters, all served to hinder the recognition of the actual occurrence in this injury—fracture. Therefore to Colles will ever be due our appreciation and gratitude for the correct diagnosis of this injury, as correct treatment in surgery is dependent upon right diagnosis even to a greater degree than in medicine.

Since the recognition by Colles of the real nature of this injury, there has been a marked tendency till within recent years to overtreat it, with more or less baneful results. Sir Astley Cooper advised that four or five weeks be let elapse before passive motion, in the aged, be attempted. He remarked that recovery is slow, it sometimes being six months before the finger motions were restored. Agnew, Bryand, Moullin, Walshe and McClelland all recognize that the stiffness of the wrist and fingers is due to fibrinous exudations around the tendon sheath about the seat of fracture. Bryand states that passive movements ought to be employed by the surgeon at an earlier period than he has been wont to; but he doesn't specify the time. Moullin advises passive movements of the fingers from the first day, and states that to prevent the stiffness of the wrist he has begun gentle manipulations on the fourth and fifth days with excellent results. The writer believes that Moullin's instructions should become the general practice. He (the writer), in several cases, after reduction of the fracture, has moulded a piece of wood-fiber splint material over the dorsum of the forearm and hand in a semi-prone position, the hand in line with the forearm. This bandaged over the parts, he finds sufficient protection for them. It does not interfere with passive motion of the fingers from the first, and is readily removed and reapplied to permit of passive motion to the wrist-joint. It is moulded directly over the skin, and requires no padding or compress. In these cases he has carefully caused passive motion of the wrist-joint from the fifth day and discarded the splint on an average of 24 days. It would appear to the writer that the treatment of "Colles' Fracture" could thus be formulated:

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* Read at the Philadelphia meeting of the American Medical Association.

1st. Reduction, not always easy.

2d. Protection, by a simple retentive appliance, while reunition of the bone takes place.

3d. Passive motion of the fingers and thumb from the first day; of the wrist, carefully, from the fifth day.

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PROCEEDINGS OF SOCIETIES.

RICHMOND ACADEMY OF MEDICINE AND SURGERY.

Regular Meeting, held May 26, 1897.

Dr. J. N. Upshur, President, in the chair.

Dr. Jacob Michaux read a paper on "Some Overlooked Points in Liver Troubles." [See page 13.]

DISCUSSION.

Dr. Landon B. Edwards said there had long been a recognized difference between jaundice and biliousness. Jaundice rarely produces material headache and is not often connected with diseases of the liver itself. In short, jaundice is a sign or symptom of obstruction of the bile ducts—whether due to catarrh of the duodenum or to the pressure of tumors or distended organs upon the bile ducts. In other words, "yellow jaundice," as people usually style it, recognizes that bile has been formed, but some obstruction along the course of the ducts prevents its excretion into the alimentary canal. The formed bile is dammed back first into the system. Circulating in the blood, it stains every tissue of the body and seeks elimination by the kidneys, causing what is known as mahogany urine. Hæmatogenous jaundice, on the other hand, refers to the retention in the blood of materials which were intended to make up the bile. In other words, it is "embryonic" bile—if such a term may be allowed—in the blood; it is not formed bile. The appearance of this hæmatogenous jaundice reminds one of that muddy, sallow color recognized as a malarial complexion or even as a cancerous cachexia, rather than of the pumpkin yellow jaundice.

As to biliousness—we all recognize its occurrence, but do not understand its pathology. It is often recognized as a bilious sick headache. Sometimes it is marked by bilious vomiting; at other times it is constipation, and sometimes there may be the absence of the usual marks of bile in the bowel discharges, but there is not the pumpkin yellow jaundice. The appearance partakes rather of the nature of hæmatogenous than hepatogenous jaundice. Apparently biliousness is due to inactive liver-cell action, but the condition is not understood. As to treatment of jaundice, due simply to catarrh of the duodenum or bile ducts, experience had taught him the value of calomel and soda. He has no theory to offer in explanation of the action, for when one undertakes to reason out the modus operandi, he is confronted with questions which he cannot answer. He simply knows it does good. This treatment is also useful in biliousness. Non-emetic or diaphoretic doses of ipecac are valuable adjuvants. Nitro-glycerin and nitrite of amyl also help. In chronic conditions, phosphate of soda is beneficial. Where there appears to be impaction of the common or hepatic ducts by bile or gall stones, he thinks he has seen beneficial results from copious draughts of olive oil. Some of the younger generation of therapeutists ridicule the supposed action of olive oil because they cannot explain it. Perhaps some of the copious draughts of oil finds its way along the walls of the common duct and even around the obstructing gall stone, thus lubricating the channel for its extrusion. If the stone should be too large to be moved by natural processes, then the surgeon will have to be called to the help of the physician.

Dr. J. S. Wellford was interested in the jaundice of the new-born. The child, when born, was red, and went gradually to white, but jaundice nearly always was present. He recognized the fact that it was somewhat due to the fœtal anatomy of the liver, which before birth discharged some of the functions of the lungs, and he was also aware of the condition, jaundice neonatorum, but he was somewhat at a loss in some cases.

A child born at the eighth month of uterine life was badly developed, weighing two or three pounds, and had an apparent absence of the fontanelles, which, however, developed in two or three days.

If there was no discoloration of the conjunctiva, he regarded the condition as normal catarrh, but, in this instance, the jaundice persisted for nearly a month.

Some years ago he had an attack of acute jaundice. well-marked and painful. It was treated most actively with saline aperients, till he had six operations daily, and he noticed that, after each operation, the skin assumed a lighter hue.

The question of biliousness can be satisfactorily explained in a number of instances: The liver was not performing its functions, e. g., making bile, and the ptomaines, instead of being eliminated, were absorbed, producing certain symptoms, among them a muddy, but not jaundiced, condition of the skin. It was a lack of elimination, seen whenever the liver was involved. In malarial regions there are always liver complications, producing the muddy complexion, etc.; and most cases of biliousness result from debauching, over-feeding, etc., and were generally relieved by saline aperients made alkaline.

Dr. Jacob Michaux, in closing the discussion, said his attention had been directed to the subject because of the confusion existing regarding terms and treatment, the former being distinctly important from both a diagnostic and therapeutic point of view. He approved everything brought out in the discussion, and reported the following case:

Woman, over 70 years of age, upon whom he operated for gall stone about ten years ago, had pain and local indications of obstruction of the common duct, followed by jaundice, which became intense. The usual remedies were given, but without avail; the urine became of a mahogany color; stools were entirely devoid of bile, etc. She was taking very little food—not enough to sustain her, as milk and rice. An operation to relieve her was done, but, unfortunately, without result. The abdomen was opened and liver and gall bladder exposed, but the endeavor to trace the duct was vain, because of the mass of inflammatory exudate which matted the omentum and intestines almost solidly, and he feared to use force. The gall bladder was incised, and with a long, probe-pointed, flexible probe he thought he could follow up the tube and push out the stone, but could not. Then he endeavored to reach the duct through the intestine and again failed. In the bladder was found a stone of considerable size. The edges of the gall bladder were stitched to those of the abdominal wound, and two small drainage tubes of India rubber were inserted, and the abdominal wound closed. For several weeks the gall bladder was washed out, but it was afterwards found unnecessary to do so, as the flow of bile through the drainage tubes was considerable, the amount reaching twelve ounces in twenty-four hours. As a result, the skin and urine cleared up, but the character of fæces remained unchanged and digestive disturbances continued.

Gaining consent for another operation, he performed one that, to his gested by reading Harley as to the establishment of a fistula. It was done knowledge, had never been done before, and that had been merely sug- in February, 1888. Openings were made in the gall bladder and a knuckle of the duodenum, and the edges stitched together. The abdominal incision was closed. The patient died on the night following the operation, and he had no further opportunity for testing its efficiency.

A point of interest was the remarkable clearing of the skin and urine, which was absolute proof of the position taken with regard to the etiology of jaundice.

Regarding biliousnesss, it appeared to be due simply to non-elimination of the materials going to form the bile.

AMERICAN MEDICAL ASSOCIATION.

Abstract of the Proceedings of the General Sessions of the Forty-eighth Annual Meeting, Philadelphia, Pa., June 1-4, 1897.

FIRST DAY—TUESDAY.

The Association met in the Academy of Music, and was called to order at 10 A. M. by the President, Dr. Nicholas Senn, of Chicago.

Prayer was offered by the Rev. L. Bradley, D.D.

Addresses of welcome were delivered by the Hon. C. F. Warwick, Mayor of Philadelphia, and Hon. Chas. Emory Smith.

Dr. H. A. Hare, of Philadelphia, Chairman of the Committee of Arrangements, announced the receptions and entertainments.

The First Vice-President, Dr. Geo. M. Sternberg, of Washington, D. C., took the chair, and President Senn delivered his annual address. He selected for his subject

THE AMERICAN MEDICAL ASSOCIATION: ITS PAST, PRESENT AND FUTURE.

He began by saying that the American Medical Association was born at the dawn of a great era in the history of medicine. Only a few years before its organization was effected anæsthesia, which has robbed the operating-room of its greatest terrors, came into general use, and at once opened up new fields of usefulness for the surgeon. The new science of bacteriology, upon which is based our modern views regarding the etiology and prevention of disease, has been founded and firmly established since that time. The principles which govern the present treatment of wounds, conceived by the immortal Lister and developed to the existing state of perfection by a host of his enthusiastic followers, have revolutionized the practice of surgery during the last quarter of a century. Normal and pathologic microscopic anatomy are recent acquisitions to our knowledge of living tissues in health and disease. Aseptic midwifery is the direct descendant of aseptic surgery and has secured for the lying-in-woman the same protection against puerperal complications as the employment of aseptic precautions will prevent largely the occurrence of suppuration, sepsis and pyemia in the treatment of the injured and patients requiring operative treatment. Anæsthesia and asepsis have created visceral surgery. Our knowledge of chemistry and physiology has been vastly increased during the last fifty years by thousands of earnest and devoted students in possession of improved instruments and apparatus for accurate investigations. During the same time great strides have been made in the practice of medicine and the preparation and methods of administration of drugs. In the light of many of these recent advancements we have at least learned that disease is influenced for the better by aiding and assisting, rather than by combating and opposing nature's resources. Translumination of the body by the wonderful Röntgen ray is the last and most important addition to our diagnostic resources in medicine and surgery. In those stirring events which have startled the medical world in such rapid succession during the last half of this century many members of our Association, dead and living, have taken a prominent and often leading part. In looking about for an appropriate subject for his address at this meeting he had deemed it expedient to utilize his time and this unusual opportunity in discussing as briefly as possible "The American Medical Association: its Past, Present and Future."

History has demonstrated the necessity of associated action in advancing the interests of the arts and sciences, commerce and the learned professions. The origin and the success of the American Medical Association have demonstrated the full meaning and force of this statement. Individual efforts accomplish but little in the correction of long-standing and widely disseminated evils. In 1835 the faculty of the Medical College of Georgia proposed the holding of a convention of delegates from all the medical colleges of the Union. This proposition met with little encouragement on the part of the remaining medical schools; and the first movement, which contemplated a convention of delegates, not only from all the medical colleges, but also from the regularly organized medical societies throughout the New Republic, was made in the Medical Society of the State of New York, at the annual session in February, 1839. At this meeting, upon motion of Dr. John McCall, it was decided to hold a National Convention in the city of Philadelphia in 1840. The movement did not meet with sufficient encouragement to carry out the desired object. In the meantime the cause of medical education received a new and vigorous champion in the person of young Dr. N. S. Davis. He was born in the State of New York, January 9, 1817. Until sixteen years of age he assisted his father in managing a farm. After receiving the limited advantages of a rural-district school training he attended for six months Cazenovia Seminary. With this scanty preliminary education he entered upon his professional duties at Fairfield, in the old College of Physicians and Surgeons of the Western District of New York, and graduated with the Class of 1837, being then but a few days over twenty years of age. Endowed with a logical mind and great power of discrimination, he became impressed during his college life with the importance of a systematic graded course of instruction. His attendance upon lectures was arranged with such an object in view, and he is probably the first graduate of an American medical college who enjoyed the benefits of a graded course of instruction, which was arranged by himself and not by his Alma Mater. Ever since his graduation as a member of the American medical profession, and as a teacher, he has been an ardent advocate of systematic graded medical instruction; and the medical college which he later founded was the first one in this country to formulate and carry out his ideas. At the time he entered upon the practice of his profession in New York, the Medical Society of that State was fostered and guided by Dr. John Stearns. Almost from the beginning of his professional career Dr. Davis became a leader among his colleagues, and a strong advocate of much needed reforms in medical education.

Medical Education.—One of the principal subjects in view of the founders of the American Medical Association was to effect a much needed reform in medical education, looking toward a higher standard of preliminary preparation, lengthening of the lecture course and a systematic graded course of instruction. These objects have been nearly attained, and largely through the influence of the Association. Nearly all of our medical schools, large and small, now require four years' attendance of eight months each, of a systematic graded course, and with few exceptions furnish adequate laboratory and clinical facilities. Our printed transactions, from the beginning of the Association until the present time, are loaded down with matter pertaining to medical education. This mission of the Association is about fulfilled. We have medical schools that are on a par with those of the older nations. There is no further excuse for our medical students to seek foreign universities to obtain a thorough medical education. Some of the very best practitioners of the United States are men who graduated in our own schools and who have never left their native soil. It requires no stretch of imagination to predict with certainty that our country will become the center of medical education within twenty-five years, and that our medical institutions will be sought by foreign nations, as they will in the course of that time furnish facilities for teaching far in advance of those of any other country. Our medical schools are not tampered with by politics; and they are undergoing a rapid evolution by acquiring unlimited financial resources and by the ceaseless activity of the wideawake, energetic and enthusiastic practitioners who by hard work and frequent intercourse with representative medical men at home and abroad become model practical teachers. The next function of the Association, as far as medical education is concerned, lies in another direction—post-graduate education.

Scientific Work of the Association.—The published "Transactions" and the volumes of the Journal of the American Medical Association are a mine of information for the general practitioner as well as the specialist, as they contain valuable contributions to medical literature, embracing all the departments of the healing art and the allied sciences. Many of the contributions have found a permanent place in our general literature and must be referred to by authors who write on the subjects of which they treat. It is a source of regret that many of the most important and interesting papers read during the early history of the Association have been buried in the volumes of the "Transactions." The value of these compendious volumes are, as a rule, underestimated by medical literary men, and are not consulted and quoted with the frequency they deserve.

Prize Essays.—Literary work in this country is appreciated only by the profession, and the only prospective reward is a consciousness of a duty performed and the recognition it may receive from the profession. Government recognition, such a strong stimulus to hard work in many of the countries abroad, is out of the question here. For the purpose of stimulating original research the American Medical Association during the first few years of its existence took the necessary steps to establish prizes for meritorious literary productions. The origin of awarding prizes for essays was from the following resolution offered by Dr. Alfred Stille, while Chairman of the Standing Committee on Medical Literature at the annual meeting in Cincinnati, in May, 1850:

Resolved, That the sum of one hundred dollars, raised by voluntary contributions be offered in the name of the Association, to the best experimental essay on a subject connected either with physiology or medical chemistry, and that a committee of seven be appointed to carry out the objects of this resolution; said committee to receive the competing memoirs until the first day of March, 1851, the author's name to be concealed from the committee, and the name of the successful competitor alone to be announced after the publication of the decision.

It will be seen from what has been said that the prize essays of the American Medical Association take a high place in American medical literature, and it must appear plain to every one that the practice should be resuscitated. Authors and investigators should be encouraged in their work. The awarding of medals and prizes will encourage the younger members of our profession to apply their talent and energy in a proper direction. The recognition by the American profession of the merits of a literary production is a source of greater satisfaction to the author than the gratification afforded by the intrinsic value of the prize.

He would recommend as the greatest stimulus to original research the awarding of an Association gold medal for the best essay on any subject pertaining to the healing art. The gold medal to be offered by and at the expense of the Association. He would suggest that one side of the medal should bear the bas-relief of the father of the Association, Dr. N. S. Davis, and the name of the American Medical Association; the other to be inscribed with the name of the successful competitor, the date of the award, with an Æsculapius staff in the center. The ceremony of awarding the medal should be in public on the last day of the meeting of the Association, preceded by an appropriate speech by the presiding officer. The competitive essays should be placed in the hands of a special committee on prize essays three months before the date of the meeting, in the same manner as was customary in competing for prizes in the past.

It is time that the profession of this city, the home of the distinguished Benjamin Rush, should do something substantial to commemorate his memory. They can do nothing better than to establish at once, before this meeting adjourns, a Rush Memorial Prize, to be awarded for the best treatise on any subject relating to the science or practice of medicine. This city has an enviable reputation for its distinguished, enthusiastic, and wealthy physicians, and he was confident they need only to be reminded of this late obligation to their most eminent colleague of the past, to step forward and subscribe the necessary fund of five or six thousand dollars. He was sure, if Benjamin Rush had a voice in this matter, he would prefer such a monument to one of marble or bronze. He hoped than in the near future every section will establish an annual prize. If this is done, we may expect a rich harvest of the most valuable contributions to indigenous medical literature.

American Medical Literature.—During the early history of our country students and physicians had to depend upon foreign authors for their text-books. With the Declaration of Independence came a sense of responsibility to our representative medical men to create a literature of our own. The first timid attempts consisted in translating or editing foreign books. The great evil of this then, and to a certain extent even now, too general a practice, did not escape the good sense and keen eye of one of the early members of this Association, whose name has become a household word throughout the civilized world—Oliver Wendell Holmes—who alludes to this subject in the following plain but significant language: "It cannot be denied that the great forte of American medical scholarship has hitherto consisted in 'editing' the works of British authors. The committee are not disposed to disguise the fact that this business has been carried on in a very cheap and labor-saving fashion. A tacit alliance between writers and publishers has infused the spirit of trade into the very heart of our native literature. The gilt letters of the bookbinder play no inconsiderable part in the creation of our literary celebrities. Sometimes the additions by the 'American editor' have been real and important, often nominal and insignificant." Dr. S. D. Gross, the Nestor of American surgery, and the foremost medical author this country has produced, raised his voice against such a parasitic literature at the meeting of the American Medical Association in 1850 by offering the following preamble and resolutions, which were unanimously adopted:

Whereas, The interests and dignity of the medical profession of the United States, as well as the true spirit of patriotism and a love of independence, demand that we should use all proper and honorable means for the establishment of a National Medical Literature, and

Whereas, We have hitherto paid too blind and discriminate a deference and devotion to European authority;

Resolved, That this Association earnestly and respectfully recommend to the medical profession generally, and to the various medical schools in particular, the employment of native works as text-books for their pupils, instead of the productions of foreign writers.

Resolved, That the editing of English works by American physicians has a tendency to repress native literary and scientific authorship, and ought therefore to be discouraged by all who have at heart the object contemplated in this preamble.

Resolved, That this Association will always hail with satisfaction the reprints in their original and unmutilated form, of any meritorious works that may emanate from the British press.

Rush Monument.—He had mentioned Benjamin Rush in his address in various connections, but he felt constrained to refer to him once more regarding a movement initiated by Dr. A. L. Gihon, to erect to his memory a suitable monument in the City of Washington. The City of Washington is embellished and graced by many statues of the men whose lives are part of the nation's history. The services of presidents, heroes, and patriots have been commemorated by the erection of statues of marble and bronze, the gift of an appreciative and grateful people. Law and science have been recognized in a similar manner by the erection of monuments, or costly oil paintings, to the memory of the leaders in the legal profession, and the exponents of science and art, as a token of respect by the legal profession and the devotees to art. He hoped the Association would erect a suitable monument to Benjamin Rush.

Association Dinner.—The Committee of Arrangements are always severely taxed in procuring suitable entertainments for the members at each annual meeting. Much time and money have been spent for this purpose; and all of us can testify to the lavish hospitality extended year after year by the local physicians and public-spirited citizens. The evening receptions have been enjoyable, and conducive in bringing the delegates and members in closer contact. But he was sure they could not take the place of an association dinner in making strangers friends, and in affording the best possible opportunities for an interchange of ideas and experiences, often of much more value than in listening to the reading of a scientific paper. Let us relieve our Committee of Arrangements of the greatest burden of their work and take the necessary steps to become independent, as far as our social enjoyments are concerned, by gathering around the festive board at our own expense. Section dinners have been a failure, and should be abolished. Let us have a dinner where the sturdy country physician can sit with his more nervous and excitable colleague from the city, where the surgeon can meet the physician, the physician with the specialists of all kinds—that is, an easy, informal commingling of the different professional elements.

Evening Demonstrations.—It has occurred to him that the third evening of each session should be devoted strictly to scientific work of a special character. It seemed to him that nothing would prove more profitable and attractive than a lecture on bacteriology, histology (normal or morbid), illustrated by magic-lantern pictures from photomicrographs projected upon a screen. The lecturer should be chosen by the Nominating Committee from year to year. We have an abundance of men well qualified for such a task within the ranks of our Association. A lecture of this nature, with numerous illustrations, would secure a large attendance, and would be one of the means of keeping our members informed of what pure science is doing for the healing art.

Permanent Home of the Association.—One of the present needs of the Association is a permanent home, with an editorial office and press-room for its official organ, a hall for the meetings (to be held at least every three years), which could also be utilized for the meetings of local societies, a library-room for American medical literature, and a memorial hall for paintings, busts of distinguished members of the Association, and a room for a collection of indigenous medical plants and surgical instruments, the invention of American physicians and surgeons. The site for such a home should be decided by vote of all members of the Association. The present financial status of the Association justifies the taking of the necessary steps to bring such a project into effect at an early date. He was sure the profession of the city that will be honored by becoming the site of such a wonderful institution will contribute liberally toward erecting and maintaining it. Such a modern Æsculapian temple would soon become the Mecca for those in search of American medical literature, and a rich storehouse for everything pertaining to the medical history of this country.

Upon motion, the thanks of the Association were extended to Dr. Senn for his instructive address, and the recommendations were referred to the Executive Committee for action.

Dr. A. L. Gihon read the report of the Committee on Rush Monument, in which he said that the amount on hand was only $4,016.19. The speaker considered Benjamin Rush one of the greatest American physicians that ever lived, one to whom American physicians owed the greatest gratitude, and who was deserving of the homage of every lover of his country because of the courage he had always exhibited and because of his martyrdom.

Dr. Cutter, of New York, moved that the Association appropriate each year $1,000 until the requisite amount was raised.

Dr. Graham, of Denver, offered an amendment that the sum of $100,000 be raised by members of the Association, and he pledged Colorado for $2,000.

This amendment was carried, after which delegates from several States pledged $2,000 each for their respective States.

Dr. Henry P. Newman, of Chicago, read his report as treasurer, in which he said that during the past year the Association had been very prosperous, in that its membership was steadily growing. There was now on hand a balance of nearly $6,000.

AMENDMENTS TO THE CONSTITUTION

were then taken up and voted upon.

Article IV.—Officers. Amend to read, "each officer shall hold his appointment for one year, and until another is elected to succeed him."

On motion, this amendment was indefinitely postponed.

Executive Council. "That there be made an Executive Council of five, consisting of three officers of the Executive Committee and two officers chosen by election. Of this council of five, one must belong to the Section on Practice of Medicine, and one to the Section on Surgery and Anatomy. To this Executive Council shall be delegated all the duties of the Executive Committee during the intervals between the meetings."

On motion, this amendment was adopted.

Article IX.—Conditions for further representation. "Any State or local medical society, or other organized institution whose rules, regulations and code of ethics agree in principle with those of this Association may be entitled to representation on the advice or agreement of the Judicial Council."

Indefinitely postponed.

To change the name of the Section on Dermatology and Syphilography to that of "Cutaneous Medicine and Surgery."

Adopted.

SECOND DAY. — WEDNESDAY.

The meeting was called to order by President Senn at 10 A. M.

The Address on Medicine was delivered by Dr. Austin Flint, of New York city, who selected for his theme

STERCORIN AND CHOLESTEREMIA.

In Hoppe-Seyler's "Zeitschrift fur Physiologische Chemie," Strassburg, 1896, is a paper by Bondzynski and Humnicki entitled "The Destination of Cholesterin in the Animal Organism." The authors profess to have discovered a new constituent of the human feces, which they call koprosterin. This substance is identical with stercorin, fully described in 1862. The reading of this article led the speaker to repeat the original researches of 1862, carrying them out by the methods then employed, at the same time repeating the observations of Bondzynski and Humnicki with the methods and appliances used in their work. It was mainly an account of these new observations that he gave. The chemical manipulations were done by Dr. H. A. Haubold, assistant to the chair of physiology in the Bellevue Hospital Medical College, and J. A. Mandel, assistant in the department of chemistry in the College of the City of New York, and to the chair of chemistry in the Bellevue Hospital Medical College. To these two skilful assistants he was indebted for most painstaking and accurate work, extending over a period of several months.

The original stercorin, of which specimens obtained in 1862 were in his possession, was extracted from the human feces by the following process: The dried and pulverized feces were extracted with ether. The ethereal extract was passed through animal charcoal and afterward evaporated. The residue was then extracted with boiling alcohol. The alcoholic extract was treated with potassium hydrate solution, at a temperature near the boiling point of water, in order to remove the fats by saponification, which were washed out with water until the filtrate was neutral and perfectly clear. The filter was dried, extracted with ether, and the ethereal extract evaporated to dryness and extracted with boiling alcohol. The stercorin was obtained from the alcoholic extract by repeated crystallization.

This process was exactly repeated in their recent observations, and, at the same time, stercorin was extracted by the process described by Bondzynski and Humnicki. Normal human feces were obtained to the amount of about fifty pounds. After drying, the feces were divided. Two analyses each were made by Haubold and Mandel, each one extracting stercorin in one portion by the original method, and in the other by the new method. All the extracts obtained were identical in their composition, reactions, and the form of crystals. It was fortunate that he had for comparison a fairly large specimen of stercorin extracted in 1862, and a microscopic slide bearing the date of June, 1862, in which the crystals were perfect. The product obtained by his process was a little more abundant and crystallized rather more readily than that obtained by the later method.

In the process employed by Bondzynski and Humnicki, the dried feces were extracted with ether, using Soxhlet's extraction apparatus. The fats were saponified with sodium alcoholate. No animal charcoal was used. The substance was purified by repeated crystallizations. These variations from the original method were unimportant, except in so far as they expedited the process of extraction. The form of the crystals and the reactions were identical with those which he obtained for stercorin in 1862. Analyses of the products obtained by them, full details of which were given in a paper sent to Hoppe-Seyler's "Zeitschrift," gave, for stercorin, the formula, $C_{27}H_{48}O$, the formula found for cholesterin being $C_{27}H_{46}O$. The change of cholesterin into stercorin was effected by the addition to the former of two atoms of hydrogen. A close comparison of the results of their ultimate analyses with those obtained by Bondzynski and Humnicki showed conclusively that koprosterin and stercorin were identical, and that stercorin was not an impure cholesterin, as is held by some eminent investigators, such as Hoppe-Seyler, K. B. Hofmann, and others. Stercorin crystallizes in long, fine needles which radiate from a center, forming tufts, and which cannot be confounded with the characteristic crystals of cholesterin. In a chloroform solution, stercorin gives, with an equal volume of concentrated sulphuric acid, first a yellow color and then a gradual change to orange, red, and finally dark red. Treated in the same way, cholesterin promptly gives a blood-red reaction without these intermediate tints.

It is now generally admitted that the bile, in addition to its function connected with digestion, contains one or more excrementitious matters. Taking into consideration the various ingredients of the bile, there seems to be but one which can logically be compared to urea. Cholesterin is found in many of the tissues and organs of the body and exists in the blood. Likening it to urea, it becomes a question whether it is formed in the liver and discharged in the bile or is merely separated from the blood by the liver and excreted. As it is constantly found in notable quantity in the nervous tissue, in the proportion of eight to twelve parts in a thousand, it occurred to him to examine the blood of the internal jugular, and compare the proportion of cholesterin with that found in arterial blood. In one experiment, on a dog, the blood being taken without using an anesthetic, he found an increase in the jugular over the carotid of nearly 60 per cent. In an etherized animal the increase was only about 3.5 per cent. In another dog, not etherized, the increase was about 23 per cent. There was also an increase of from four to six per cent. in the blood of the femoral vein over arterial blood. In three cases of hemiplegia, the blood from the arm of the sound side contained about the normal proportion of cholesterin, while blood from the affected side contained no cholesterin.

Passing from these observations to the pathological relations of cholesterin, after examining three specimens of normal blood, and finding the proportion of cholesterin from 0.445 to 0.751 of a part in a thousand, examinations were made of the blood of patients with simple jaundice and those with what is called icterus gravis, the cases terminating fatally with grave nervous symptoms. In a case of simple jaundice, terminating in recovery at the end of about four weeks, the blood contained 0.508 of a part in a thousand, well within the limits in normal blood. In a case of jaundice with cirrhosis, terminating fatally with serious nervous disturbance, the blood taken six days before death contained 1.850 parts in a thousand of cholesterin, an immense increase over the normal proportion. In this case, on post-mortem examination, the liver was found contracted, and the gall-bladder was shrunken, containing only about seven cubic centimeters of bile.

The question of cholesteremia has been much discussed since 1862, for the most part with scant approval or without acceptance. However, Picot, in 1872, reported a fatal case of "grave jaundice," in which he determined a great increase in the proportion of cholesterin in the blood, 1.804 parts in a thousand. Many attempts have been made, also, to produce toxic effects by injecting cholesterin into the blood, but most of them have been unsuccessful on account of mechanical obstruction of the blood-vessels. In 1873, however, Koloman Müller succeeded by injecting cholesterin rubber with glycerin and mixed with soap and water. In five experiments on dogs, injecting in each 0.045 gramme of cholesterin, he produced a complete representation of the phenomena of "grave jaundice."

In repeating the original researches of 1861, the observations, as regards analysis of feces, etc., were somewhat extended. With modern apparatus, the manipulations may be freed from many disagreeable features which heretofore, probably, have interfered with this line of investigation. In extracting stercorin, various volatile fatty acids and other substances were removed, the constitution and relations of which are unknown. They studied, in this connection, some of the products of bacterial action, obtaining, by the action of fecal bacteria on proteids, skatol and indol, both substances containing nitrogen. It is well known that phenol and cresol also exist in the feces. These nitrogenized matters are putrefactive products; nothing is known of their physiological or pathological relations; and up to this time stercorin is the only excrementitious matter yet found in the feces the origin and relations of which are at all understood. Our knowledge, indeed, of the physiological chemistry of the feces is only just begun; and we may look to future investigations for much that will be most important as well as interesting. The same might be said, in a measure, of the bile and of the true pathology of certain functional and structural diseases of the liver. How long shall we continue to speak of biliousness, congestion, or torpor of the liver, the classic "liver complaint," et id genus omne, using terms which have no scientific meaning? Undoubtedly there are general disturbances, dependent upon some disorder in the functions of the liver, which occur without jaundice; and this fact has long been recognized. In a case of cirrhosis with considerable constitutional disturbance but no jaundice, the blood was found to contain an excess of cholesterin 0.922 of a part in a thousand. In what is termed acholia, there may be grave nervous symptoms without jaundice; and the pathology of such cases is unknown. The biliary salts are not found in the blood, and the symptoms cannot be accounted for by disturbances in digestion. It is possible that light will be shed upon their pathology if it is admitted that there is a condition called cholesteremia. As yet this is but speculation; but if the theory of cholesteremia is accepted, a wide field of inquiry is opened in this direction, and ere long we may speak of "biliousness" and "liver complaint" with some definite ideas of their pathology.

With this paper he presented photographs of cholesterin, stercorin extracted by the original method, and stercorin extracted by the method of Bondzynski and Humnicki, all in 1897, with a photograph of crystals obtained in 1897 from a specimen of stercorin extracted in 1862.

President McKinley was then introduced, and addressed the Association for one or two minutes. He said he could not refrain from pausing a moment that he might come into this brilliant presence to meet the learned gentlemen assembled, and to pay his respectful homage to the noble profession which they so worthily represented.

Governor Hastings followed with a few remarks in which he considered that the development of medicine had been greater than that of any other science. He thought it a mistake that monuments were not erected to the great masters of medical science in this country.

Dr. H. C. Wood, of Philadelphia, spoke forcibly on the Anti-vivisection Bill, and after giving a brief history of past legislation on the subject he emphasized the necessity of each member taking a personal interest in opposing the sinister and persistent efforts of the anti-vivisectionists. He offered a resolution against the passage of Senate Bill No. 1063, which was unanimously adopted.

THIRD DAY. — THURSDAY.

The Association was called to order by President Senn at 1 o'clock.

Dr. Frederick Horner presented a report from the Committee on Organization of a Relief Association, and it was moved that the President appoint a committee of one from each State to establish such an association.

The report of the Board of Trustees showed a satisfactory condition of the treasury, and also a steady increase in the circulation of the Journal.

Dr. W. W. Keen, of Philadelphia, was then introduced, and delivered the

ADDRESS IN SURGERY.

He said: "As we celebrate on this occasion the semi-centennial of the organization of the American Medical Association in this city in 1847, it is very natural and proper that the Address in Surgery should be a review of the work done in the last fifty years, and by contrasting the state of surgery and of surgical teaching in 1847 with that which exists in 1897, to see what progress has been made. To account what had been achieved in these 'fifty years of science,' far better than a 'cycle of Cathay,' is not only a pleasure, but it is an immense incentive, since, by the progress made in the last fifty years, we can in some measure anticipate the enormous and probably even still greater progress that will be made in the next half century.

"A most important factor in the improvement, not only in surgery, but in all departments of medicine, has been the immense advance made in medical teaching. The educational plane of the profession has been steadily elevated. If the teachers of fifty years ago were to revisit the scenes of their early labors, they would scarcely recognize the medical colleges in which in their day and generation and with the meager appliances then at their command, they did what we must still recognize as yeoman's work in education. Apparently at that time the entire instruction consisted in lectures, no text-book being even advised. In reply to the letter addressed to the deans of the Jefferson Medical College, the University of Pennsylvania, Harvard University, and the Medical Department of Columbia University, I am told that no lists of text-books whatever appear on the catalogues of fifty years ago.

"Now, we may congratulate ourselves that the majority of the medical schools of the country have a graded course of four years, each covering not less than six and often eight months; not only lectures, but in many instances constant and searching recitations; almost a score of laboratories, in which each student actually does the work of observation and experiment; ward classes, in which every man is obliged to train his eyes, his ears, his fingers, and his judgment in the examination of patients in every department of medicine, to ferret out the history of the cases brought before him, ascertain symptoms, seek for physical signs, reach a diagnosis, determine the treatment, and often actually to prescribe and to assist at operations.

"Allied to medical teaching and the most important adjunct to medical literature is the establishment of extensive medical libraries. In this, as an American, I am proud of my own country. No foreign nation can point with equal pride to any such medical libraries as the last thirty years have developed in this country. Foremost, not only among American libraries but in the world is that of the Surgeon-General's Office of the United States Army in Washington. Not only has it gathered thousands of medical books and the 1,100 medical journals (the estimate of Dr. Fletcher) from all over the world, but the entire library is managed with a liberality which makes it the admiration and the envy of foreigners. Its treasures are freely at the service of the entire profession of the country; and the publication, under the editorship of Dr. John S. Billings, of its magnificent 'Index Catalogue' has made the whole world debtors to America. We trust that a more liberal Congress may see that if even the small amounts thus far given to it have made it of such immense value, still larger and more generous appropriations would keep it ever in the van."

Dr. Keen then proceeded to dilate upon other noteworthy features upon which the development of modern surgery is dependent, declaring, among other things, that without anæsthesia and antisepsis, modern surgery would be an impossibility. Animal experimentation, he said, had had also a very large share in the development of modern surgery. The whole question of the introduction of animal ligatures was begun in America by Physick, who used buckskin; his follower, Dorsey, used kid, and cut both ends short; Hartshorne used parchment, and Bellenger and Eve used the tendon of the deer. The problem has been solved principally by experiment upon animals in order to determine accurately the behavior of such ligatures in the tissues. Only professional readers can appreciate what a boon to humanity this simple achievement has been. Modern cerebral surgery also owes its exactness and success almost wholly to cerebral localization and antisepsis, both of which were first studied by experiment upon animals, and later by the application of the knowledge so gained to man. Bacteriology would not now exist as a science, nor would accurate, modern surgery and a large part of modern medicine be possible had experiments upon animals been prohibited, as some zoophilist women, who love dogs better than men and women and even little children, desire.

Dr. Keen reviewed the surgical treatment of various cancerous diseases of the organs; and of appendicitis he said: "The appendix, that meager but most troublesome ancestral vestige, which with the bicycle has been the faithful friend of the surgeon through the past four years of commercial depression, has been recognized within the last few years as the real origin of the so frequent abscesses in the right iliac fossa. Beginning with Willard Parker's paper in 1867 and Fitz's memorable paper in 1886, the treatment of appendicitis, and even its much abused name, are distinctly of American origin and an immense credit to American surgery.

"A hasty review, such as has been given of the improvements in surgery within the last fifty years, does much more than show us the adroitness, audacity, and success of the modern surgeon.

"That is the thing which strikes us most as surgeons; but we must regard all these improvements also from the side of the patient and the family, and see what it means. It means a prolongation of life by operations which, while not without pain and suffering during recovery, have been robbed of their primary terrors by anæsthesia, and of most of their subsequent pain and suffering by antisepsis; it means that patients who in 1847 were hopelessly consigned to the grave after weeks and months of suffering are now, in the vast majority of cases, rescued from death; it means that families formerly bereft of husband or wife, parent or child, and left to spend years of sorrow, of suffering, and, in many cases, of poverty because the breadwinners were taken away, have now restored to them their loved ones in health and strength and usefulness; it means that the hecatombs of a Cæsar, of an Alexander, a Napoleon, are offset by the beneficent labors of a Morton, a Warren, a Lister, who are—and who for all time will be—blessed by many a poor patient who never heard of them, instead of being cursed as the destroyers of nations and of homes innumerable; it means that man's inhumanity to man shall be replaced by a scientific and Christian altruism which sheds blessings and benefits on the whole human race, seeing in the patient, whether saint or sinner, only a human being who is suffering from accident or disease, whom it is the province of the surgeon, in imitation of Him who went about doing good, to restore to health and happiness. Even where life cannot be prolonged, the agonies of death itself can be soothed by his hand and fruitful skill.

"What the future has in store for us we can only dream. Two diametrically opposing tendencies are prominent in modern surgery: radical interference with disease, so that there is scarcely now a single organ or portion of the body not within our reach; yet, on the other hand, a remarkably conservative tendency in cultivating remedial rather than radical surgery. Joints so diseased as once to require amputation are now treated conservatively with the best results. Ovaries, a portion of which can be preserved, are kept in the abdomen; kidneys once doomed to extirpation are now partially removed, and bones so diseased that they then required amputation are now excised and the limb preserved. Experiments upon animals have recently given us wholly new views of infection and of the origin of many diseases, and also the little knowledge that we yet have as to either natural or acquired immunity and to a consequent orrhotherapy."

It is, he believes, on these lines that our more immediate future triumphs will be achieved. We have discovered the actual cause of tetanus, tuberculosis, erysipelas, suppuration, and a host of other diseases and conditions, of the causes of which we were wholly ignorant a few years ago. The causes of many other disorders, both medical and surgical, still remain hidden from our view. We know almost nothing of the origin of benign tumors, and are groping to discover the origin of cancer, sarcoma, and other malignant growths. When we have discovered the cause we are nearly half way, or, at least, a long way, on the road to the discovery of the cure; and I think it not unlikely that in 1947 your then orator will be able to point to the time when a definite knowledge of the causes of these diseases was attained, and probably to a time when their cure was first instituted.

That will be a surgical paradise when we can lay aside the knife and by means of suitable toxins or antitoxins, drugs, or other methods of treatment, control inflammation, arrest suppuration, stay the ravages of tuberculosis or of syphilis, abort or disperse tumors, cure cancer, and, it may be, so prolong human life that all of one's then audience will die either of accident or of old age. Would that you and I could be alive in 1947 to join in the glorious surgical Te Deum.

JUBILEE EXERCISES.

The founder of the Association, Dr. N. S. Davis, of Chicago, then made his appearance on the stage, escorted by the presidents of the various State medical societies and of the State Boards of Medical Examiners.

Dr. John B. Roberts, Chairman of the Committee on Anniversary Exercises, in a very appropriate speech presented Dr. Davis to President Senn.

Dr. Davis was very warmly received on rising to speak. He read an address entitled

"A Brief History of the Origin of the American Medical Association, the Principles on which It was Organized, the Objects It was Designed to Accomplish, and How Far They have been Attained During the Half Century of Its Existence."

Dr. John B. Roberts said that diligent search had revealed that there were only four of the original members of the Association still living.

Dr. George Ben Johnston, of Richmond, Va., followed with an address entitled: "What the State Medical Societies have done for the People."

He said it was worthy of note that every State in the Union had to-day its medical society, and that, although their ages differed, all were filled with the same spirit and vigor of youth. In most States, there was formerly no requirement for a license to practise beyond the mere payment of a tax; but to-day all was changed, and now there was scarcely a State in which there was not a sufficient and well-executed law by which only qualified men were admitted to practise. The enactment of such laws as now exist was procured through the efforts of the State societies acting independently of each other; and there never was any well-directed attempt at co-operation, which accounted for the dissimilar character of the various statutes and the varying nature of the prescribed machinery for executing them.

DEATH OF DR. COCHRAN.

Resolutions were read and adopted regarding the death of Dr. Jerome Cochran, of Alabama.

(*To be continued.*)

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THERAPEUTICS.

DIRECTIONS FOR THE USE OF GLUTOL-SCHLEICH.

Wounds may be divided into four classes, in each of which Glutol must be employed in a special manner:

1. Fresh traumatisms and operative wounds in which the Glutol treatment is begun within the first few hours after the solution of continuity.

2. Wounds which have been exposed to the possibility of infection for more than four hours.

3. Inflamed and suppurating wounds.

4. Larger losses of substance and ulcerations.

1. In recent wounds the Glutol must be brought into intimate contact with the whole of the raw surface. In incised wounds the opposing surfaces must be well powdered with the material, and an extra quantity gently pressed in between their margins. In irregular and lacerated wounds it must be carefully rubbed into all the cavities and corners of the lesion. Portions of tissue that are manifestly incapable of recuperation must be removed; yet oftentimes endangered fragments, such as portions of skin and bone, recover their vitality under the Glutol treatment. All wounds containing necrosed material, however, must be carefully watched.

The scab that is formed may be allowed to remain in situ until healing occurs.

2. In older injuries the primary Glutol dressing must be renewed after 24 hours in every case.

3. Inflammatory and suppurating wounds must be treated according to general surgical principles before the Glutol is applied, so that sufficiently healthy tissue may come in contact with the Glutol. Incision, drainage, cleansing of the wound, removal of the dead tissue fragments, etc., must be employed as indicated. If a scab forms it must be removed in some places at all events, during the first 48 hours, to enable fresh Glutol to come in contact with the denuded surfaces.

4. With ulcerated areas the preliminary removal of all dead or nonviable material is of the greatest importance, and here also the scabs must be removed from time to time.

Burns in any stage can be quickly healed with Glutol.

Where the suppuration is free it is well to remove the hardened or more or less fluidified Glutol from time to time, and apply fresh material.

The great advantage of Glutol lies in the absolute certainty of autodisinfection of the wound by the tissue cells themselves. This auto-disinfection occurs in infected wounds also, as soon as neighboring and healthy cell material comes in contact with the Glutol in sufficient quantity. The greatest care must therefore be taken to open up all infected cavities. Tortuous wounds and suppurating sinuses may be treated with the Glutol tampon.

The layer of Glutol must always be covered with aseptic gauze except in the case of small wounds, where the scab formation occurs in a very few hours.

Berlin, December, 1896. Dr. C. L. Schleich.

The grated form will hereafter be supplied exclusively, as Dr. Schleich has found it to give better results than the fine powder.

THE ANTITOXIN TREATMENT OF DIPHTHERIA.

Dr. Floyd M. Crandall, of New York, N. Y., says in the Medical Fortnightly:

The antitoxin treatment of diphtheria is well past the stage of probation. It is true that it still meets with vigorous opposition, which arises in some quarters, from very competent and experienced practitioners. Until all men, however, join one church and one political party, and agree upon the tariff and silver questions, we must expect differences of opinion upon medical questions, and particularly upon treatment. When schools of medicine disappear, and all physicians practice alike, adopt the same course of treatment, and approve the same drugs, we may expect unanimity regarding vaccination and antitoxin.

Evidence regarding the efficacy of the serum treatment has attained such proportions, that it cannot be lightly dismissed with the statement that men have been mistaken in their observations, or that the results are due to carbolic acid or other antiseptic. This evidence comes from thousands of observers in Europe, America and Australia, and covers a period of almost three years. It cannot, therefore, be attributed either to personal prejudice, local conditions or mildness of type of the prevailing disease. The evidence was at first based chiefly upon hospital experience. Notwithstanding its favorable character, it was regarded as not wholly satisfactory, for the hospital test is, for various reasons, not the most reliable in a disease like diphtheria. During the past year results of treatment in private practice have been reported in large numbers. The most notable of these is the report of the American Pediatric Society, which has received very wide reading. This report has very properly been considered of great scientific value, compiled as it was from most carefully sifted data by men like Holt, O'Dwyer, Northrup and Adams, whose integrity and honesty of purpose cannot be doubted, and whose ability to handle statistics cannot be questioned.

While some of the evidence presented during the past two years has been conflicting, and some has been unfavorable, the conclusion cannot be avoided, that, taken as a whole, it overwhelmingly proves that we have in antitoxin a most potent remedy against diphtheria. One of the most positive proofs of its value is the almost universal approbation it receives from the men who have had the largest experience with it in private practice. It should be judged by the experience of men who have used it early in adequate doses in a large number of cases, and not by the experience of those who have used a dilute serum timidly in a few cases.

The evidence now at hand as to the great potency and practical value of the serum treatment is so complete that it certainly seems that it were better in the fuure to devote less time to debating this question, and to devote more time to the study of its applications and dosage. A few suggestions in this direction may, therefore, be appropriate.

The doses should always be measured in antitoxin units, for the amount of serum injected is no indication of the amount of antitoxin used. The strongest serum available should always be employed. The serum is only a vehicle for the antitoxin; some of the unfavorable symptoms reported have unquestionably been due to the serum rather than to the antitoxin. It is desirable, therefore, to reduce the amount of serum to the lowest possible point. If available, serum containing 500 antitoxin units to the cubic centimeter should be employed. The amount of serum used is thus but one-fifth that employed two years ago. This permits the use of a small syringe. Both the syringe and the needle should be surgically clean, and the skin at the site of injection, as well as the operator's hands, should be carefully disinfected. The point of puncture should be sealed with collodion, and every precaution should be taken to avoid infection from extraneous sources.

The subject of dosage still requires much study. In general terms, it may be said that the doses now administered are somewhat larger than those employed a year ago. The following rules recently laid down by the American Pediatric Society, after very careful consideration, cannot, in the present state of our knowledge, be improved:

"For a child over two years old, the dosage of antitoxin should be in all laryngeal cases with stenosis, and in all other severe cases, 1,500 to 2,000 units for the first injection, to be repeated in from eighteen to twenty-four hours if there is no improvement; a third dose after a similar interval if necessary. For severe cases in children under two years, and for mild cases over that age, the initial dose should be repeated as above if necessary; a second dose is not usually required."

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MISCELLANEOUS.

OVARIAN OPOTHERAPY.

C. Jacobs, of Brussels (La Policlinique; Brit. Med. Jour.), reports the results which he has obtained from the use of ovary juice in various diseases of women. The cases in which he has used this substance are grouped by him as follows:

1. A series of patients in whom partial or complete removal of the genital organs had been performed by the abdominal or vaginal route, and in whom various disturbances of the kind associated with the menopause or dysmenorrhea had developed.
2. Patients who had reached the age of the natural "change of life," and who presented troublesome symptomatic phenomena.
3. Anæmic young girls .
4. Women suffering from amenorrhea due to incomplete development of the genital organs, or from dysmenorrhea due to an arrest of the development of the internal genitals.

Ovarian gland substance was given: first, in the natural state—in this form it is taken with difficulty; second, in powder, after desiccation of the organ—in this form many patients cannot digest it; third, in glycerinized or liquid extract, in form of "oophorine wine," the basis of which is extract of cow's ovary. This is generally well borne in a daily dose of 20 grammes, corresponding to 20 centigrammes of the extract. Details are given of 51 cases, 34 of which were under the author's own observation, 15 under that of Dr. Raulier, and one under that of Dr. Balteaux, of Wavre. From the results Dr. Jacobs formulates the following conclusions:

1. The troublesome symptoms of the natural menopause disappeared or were greatly diminished by the use of the extract of ovary without any other medication.

2. Similar effects were produced by the administration of that substance in the relief of symptoms—for instance, irritability of the bladder—that follow surgical operations which have for their result the suppression of the menstrual flow.

3. Rapid improvement is constantly seen in chlorosis and dysmenorrhea.

4. The influence of extract of ovary on the psychical disturbances which accompany or are dependent on genital lesions are undeniable.

5. Rapid and permanent improvement in the general state.

6. Climacteric metrorragia without neoplastic lesions yield rapidly to the administration of the remedy.

7. Its therapeutic action on the nervous system is manifest from the first day of its administration.

Jacobs states that he will shortly publish the results of laboratory researches as to the chemical constitution of the substance which he prescribes.

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REVIEWS.

Dercum on Nervous Diseases. A Text-Book on Nervous Diseases. By American authors. Edited by F. X. Dercum, M.D., Clinical Professor of Diseases of the Nervous System in the Jefferson Medical College, Philadelphia. In one handsome octavo volume of 1,052 pages, with 341 engravings and 7 colored plates. Cloth $6.00; leather, $7.00.

List of contributors: N. E. Brill, M.D.; Chas. W. Burr, M.D.; Joseph Collins, M.D.; Charles L. Dana, M.D.; F. X. Dercum, M.D.; Geo. E. de Schweinitz, M.D.; E. D. Fisher, M.D.; Landon Carter Gray, M.D.; C. A. Herter, M.D.; George W. Jacoby, M.D.; William W. Keen, M.D.; Philip Coombs Knapp, M.D.; James Hendrie Lloyd, M.D.; Charles K. Mills, M.D.; S. Weir Mitchell, M.D.; Charles A. Oliver, M.D.; William Osler, M.D.; Frederick Peterson, M.D.; Morton Prince, M.D.; Wharton Sinkler, M.D.; M. Allen Starr, M.D.; James C. Wilson, M.D.

Lea Brothers & Co., publishers, Philadelphia.

Foster's Physiology. Text-book of Physiology. By Michael Foster, M.D., F.R.S., Prelector in Physiology and Fellow of Trinity College, Cambridge, England. New (sixth) American edition with notes and additions. In one handsome octavo volume of 922 pages, with 257 illustrations. Cloth, $4.50; leather, $5.50. Philadelphia, Lea Brothers & Co., publishers.

Gray on Nervous and Mental Diseases. New (second) edition. A treatise on nervous and mental diseases. By Landon Carter Gray, M.D., Professor of Diseases of the Mind and Nervous System in the New York Polyclinic. In one very handsome octavo volume of 728 pages, with 172 engravings and 3 colored plates. Cloth, $4.75; leather, $5.75. Philadelphia, Lea Brothers & Co., publishers, 1895.

Hare's Text-Book of Practical Therapeutics. A Text-Book of Practical Therapeutics; with Especial Reference to the Application of Remedial Measures to Disease and their Employment upon a Rational Basis. By Hobart Amory Hare, M.D., Professor of Therapeutics and Materia Medica in the Jefferson Medical College of Philadelphia. With special chapters by Drs. G. E. de Schweinitz, Edward Martin and Barton C. Hirst. New (fifth) edition, thoroughly revised. In one octavo volume of 740 pages. Cloth, $3.75; leather, $4.75. Philadelphia, Lea Brothers & Co., publishers, 1895.

Green's Pathology and Morbid Anatomy. Pathology and Morbid Anatomy. By T. Henry Green, M.D., Lecturer on Pathology and Morbid Anatomy at Charing-Cross Hospital Medical School, London. Seventh American from the eighth and revised English edition. Octavo volume of 595 pages, with 224 engravings, and a colored plate. Cloth, $2.75. Philadelphia, Lea Brothers & Co., publishers, 1895.

Hayem & Hare's Physical and Natural Therapeutics. Physical and Natural Therapeutics. The Remedial Use of Heat, Electricity, Modifications of Atmospheric Pressure, Climates and Mineral Waters. By Georges Hayem, M.D., Professor of Clinical Medicine in the Faculty of Medicine of Paris. Edited with the assent of the author, by Hobart Amory Hare, M.D., Professor of Therapeutics in the Jefferson Medical College of Philadelphia. In one handsome octavo volume of 414 pages, with 113 engravings. Cloth, $3.00. Philadelphia, Lea Brothers & Co., publishers, 1895.

Fuller on Male Sexual Disorders. Disorders of the Sexual Organs in the Male. By Eugene Fuller, M.D., Instructor in Venereal and Genito-Urinary Diseases, New York Post-Graduate Medical School. In one very handsome octavo volume of 238 pages, with 25 engravings and 8 full-page plates.. Cloth, $2.00. Philadelphia, Lea Brothers & Co., publishers, 1895.

GAILLARD'S MEDICAL JOURNAL.

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Scientia et Veritas Sine Timore.

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EDITORIALS.

MEDICAL ENGLISH "AS SHE IS WRIT" IN GERMANY.

The enterprise of the numerous German manufacturers of chemicals is rapidly developing. Doubtless encouraged by the success of the American manufacturer, his German colleague now resorts to advertising by circular in a most extensive manner.

Although such "circulars" are usually thrown into the waste basket and do not therefore reach the eye of so large a number of practitioners as do announcements in the Medical Journals, the manufacturers continue to pour these missives into the offices of the long-suffering doctor. A recent circular, announcing the value of a certain pharmaceutical preparation, has afforded us so much amusement that we offer some excerpts from it to our readers to compensate them for being bored:

"If from this case like many others, treatment allows to make a sure conclusion, it invites strictly to make more experiments. An application in private practice I will surely use in appropriate cases."

"The leaves after the production of the fruits had a containing of sugar."

"Who has opportunity to witness many dissections will literally get used to it, to find, by dissections for other causes of deaths, by a large percentage, at least in half of the cases, as occasional and incidental observations, old, healed, tubercular hearths."

"It is put out of all doubt, that in its initial stades, tubercular does not absolutely need to take a deadly course, but very often even has an inclination of healing over."

"This endeavor to assist the nature is business of the doctor."

Among the cases described we find the following:

"Mathias, wife, 20 years old, since years constipated nose, discharge of bloody, mattery, stinking barks."

"After my experiences this case only enters with persons of sitting conduct of life without any movement I renounced to give a purging addition; a teaspoonful of 'Salt of Carlsbad' taken sober in the morning."

"Maidservant H. Both nose shells polypous bubbly swoln, speaks through the nose, oznena, lachrymol nose channels constipated."

"Affeine to take the medicine regularly. As this is, from my side, on account of the resultlessness, directly denoted to be a lie."

"Abdomen strongly meteoritically driven up. Liver suffocation not observable. The excretion becomes less prudent, contains tubercular bacillusses."

"This form has an unagreeable disadvantage, it attacks the teeth in a bad way by etching."

THE STUDY OF THE BLOOD.

A recent excellent paper in the Pacific Medical Journal, by Dr. J. A. Sanderson, of St. Helena, California, entitled "Practical Value of the Study of the Blood," suggests a number of points of material use to the every day practitioner. He begins by the simple statement that in view of the important relation of the blood constituents to the general nutrition, as well as to the embryological development of the body, it is not unreasonable "to expect to find in its close examination many things indicative of the real conditions that are present in the various phases of health and disease; and it is quite surprising how slowly the pathology, or even the physiology of the blood has advanced in comparison with other branches of medical science." Then follows the statement that the "facts that are of practical importance concerning the blood can be determined, first, by the study of the fresh specimen; second, by having the per cent. of hæmoglobin; third, by the estimation of the white and red corpuscles; and, fourth, by the examination of the dried and stained specimens." "A few drops of blood from the end of the finger or lobe of the ear, preferably the latter, are sufficient to make any one or all of these examinations." Then follows the facts in regard to the number and kinds of corpuscles, their proportion and the significance of their disproportion; the presence of parasites, the one of especial importance in this country being the plasmodium malariæ. We are not so sure that the count of the corpuscles, even in the hands of experts, can always be accepted as accurate; but we can easily note a disproportion and from such a condition draw important conclusions.

The point that we would particularly emphasize, is that in every case, as a matter of routine, it is not unwise for us to make an examination of the blood. This is particularly true of indefinite fevers, in which the discovery of the malarial parasite gives us a definite diagnosis, and suggests the proper plan for therapeutic attack.

FOREIGN STUDY.

In the Journal of the American Med. Asso., June 19, '97, appears an editorial entitled "The Foreign Cult in American Medicine," and it voices so many sentiments of practical value that we feel compelled to quote the same in full.

"The latest published utterance of a recently deceased American naturalist, who was second to none in the specialties he had adopted, and was furthermore one of the most philosophic and broadest in his culture, was a protest against a tendency that appears to exist among scientific students to overestimate the value of the study of the infinitesimal and the culture of Europe as compared with our own. If this is true amongst scientific men it is much more so in medical circles, and his essay may well serve to point a moral for the consideration of our profession. At the present time it is still the fashion to consider no medical education complete without a visit to European medical centers, and there is a certain prestige to a practitioner who has 'hospitirt' in the lecture rooms of Berlin or Vienna, or other transatlantic university city. It is assumed that wisdom has been absorbed, whether the sojourn was brief or lengthened, and the question of acquaintance with the language, or of actual study, is seldom or never raised. There are many who claim this prestige who would fail to understand twenty-five consecutive words of a lecture delivered in any other tongue than their own, and whose attendance might have been as well in Moscow or Bucharest as in Berlin or Paris, for any good it has done them. There are others, somewhat better fitted, who enter laboratories and pick up a certain amount of knowledge, which they might have better learned in their own country, and ever after claim the distinction of an acquaintance and familiarity with the great lights of medicine abroad, that they turn to their financial profit by advertising it amongst a wondering clientèle. Having too often had no experience, and leaving their own land in the callow state of recent graduation, they are liable to only imbibe one-sided and false notions, to the disparagement of their country and its scientific standing, the more so, since by exploiting these on their return they think they contribute to their own glory by shining in the reflected light of their illustrious European acquaintances. There is, of course, another class who honestly admit that a European trip is more for pleasure than business, and a certain number who actually utilize it adequately and estimate it at its proper worth. These, however, are largely special students, and often those who have already done scientific work that has given them a standing, it may be, even abroad. It is abundantly true that an American who is properly fitted can learn something abroad, as the converse is also true that the European could learn something here, if he could only get that fact into his consciousness. Too many, however, of the freshly made doctors, who finish off with a European trip, derive from it no intellectual benefit whatever; their principal acquisitions are liable to be a certain deterioration of morals and a large amount of additional conceit. In time, under wholesome home influences, this will probably wear off, but it is no advantage to have had it exist and it must leave to a proper minded individual an unpleasant reminiscence.

"With those who really profit by European study no one would need to quarrel, but for the fact that even among these there are some who, through want of mental balance or some other defect, come back with an undue estimate of themselves, and a disparaging opinion of their own country and countrymen. A 'certain condescension in foreigners,' humorously noted by Lowell, can be endured, recognizing the fact that it is generally a characteristic of second rate individuals, but to have it encouraged by the peculiarities of our own compatriots is not at all gratifying, and is much less endurable. It almost makes a little medical jingoism seem desirable to counteract the silly egotism that tries to magnify itself at its country's expense.

"Professor Cope's protest against the undue magnification of microscopic research is also valid in medicine; we have the same tendency to correct. Section cutting and staining learned abroad no more makes a pathologist than it does a biologist, and it is in medicine, as in science, not less really scientific to observe properly with the naked eye than 'through a brass tube furnished with lenses.' The overestimation of the latter method is responsible for the waste of much patient work and expense, and this is especially true in medical science. What we need is not less careful pathologic work, but more and better clinical observation to guide and interpret it, and it is easier to make a trained microscopist than a really skilled clinician. As it is, however, it often happens that a manual skill and dexterity in microscopic work is valued above far more important and difficult accomplishments, simply because it is more showy and requires the use of elaborate paraphernalia that are beyond the reach of the many. There has been a large amount of imposition upon the medical public in this way, as is shown by the records of much so-called pathologic work in the past. It is to be hoped that there will be less of this in the future, but this result will be brought about more by a correct appreciation of the greater value of good and thorough clinical observations than by anything else. It is worth while to occasionally emphasize this truth.

"There is a possibility of becoming eminent in medicine without a European polish to our professional education, but there is a slowness in certain quarters and certain individuals to recognize this fact. Indeed it is even now a matter of surprise and comment amongst some high European authorities that there should be such a lack of recognition, but it is not their interest to complain. The time, however, will come when American centers of medical education will draw European students, and at the present rate of progress it may not be so very far distant. The sooner it appears the better, and we can best aid its coming by discouraging the dishonorable depreciation of what is really good in our own country, and the too prevalent cult of a foreign fantasy in matters of medical science and education."

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ORIGINAL ARTICLES.

DIGESTIVE FERMENTS.*

By MARK W. PEYSER, M.D., of Richmond, Va.

Lecturer on Physiology, University College of Medicine; Secretary Richmond Academy of Medicine and Surgery.

For some time I have been of the opinion that, among other agents, the digestive ferments were, perhaps, used more injudiciously than any remedy in materia medica, my experience being that the faulty work of the alimentary canal was due rather to lack of acid and carbonate of sodium, borne out by the relief obtained through the use of these.

Because of this, the ferments of various manufacture have been often decried by the practitioner, when the fault lay in his own diagnosis.

In the past year much light has been thrown upon the physiological action of enzymes; so much, in fact, that it would be beyond the limit assigned to this sketch to enter into detail; but it is desired to mention one fact of vast importance in every practice. It has been until recently taught that as soon as food was introduced into the mouth, the flow of gastric juice was excited, the stomach thereby becoming acid in reaction. Because of this, salivary digestion ceased when food was swallowed, ptyalin, acting only in neutral or alkaline media, being destroyed. Also, because of this, it was almost a waste of time and material to administer pancreatic enzymes, which act, too, in alkaline media, and which are obliged to run the acid gauntlet of the stomach.

It is now, however, known that the acid gastric juice is not secreted for some appreciable time after food has passed into the stomach; that ptyalin continues to act after it has been swallowed, further changing the intermediate products of salivary digestion into maltose; and that for this reason we may be reasonably certain of obtaining desired results when the pancreatic enzymes are given by the mouth.

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* Read before the Richmond Academy of Medicine and Surgery, June 22, 1897.

But, as was said before, disappointments are likely to follow because we do not discriminate between lack of acid and lack of pepsin; between lack of carbonate of sodium and lack of trypsin, or amylopsin.

The test tube is by no means a criterion of what occurs in the human economy; but I should like to relate an impressive fact as to the stability of the various digestive ferments on the market; and, before going further, let me state that I do not desire to make any invidious comparisons nor champion a particular manufacturer.

In making preparations for demonstrating the action of the digestive ferments, before the first year class of the University College of Medicine in physiology, I could find but three preparations of the necessary agent at hand. Two of these were not much less than three years old, and one, that of Parke, Davis & Co.'s lactated pepsin, had been unstopped for nearly that length of time; the second preparation of pepsin had never been opened, and retained to the naked eye the characteristics it had when first received. The third specimen was a liquid preparation that had been left at the office but a few days previous, and, therefore, to be supposed, perfectly fresh.

All were subjected to the same test. Three beakers were taken, and in each was placed a piece of coagulated egg-albumen, weighing two drachms. In beaker A was poured a solution of lactated pepsin, in beaker B a solution of the second preparation, with 2 per cent. solution of hydrochloric acid; in beaker C, the third, the liquid preparation.

A word of explanation is now in order. It is claimed by the manufacturers that lactated pepsin contains all the ingredients necessary for gastric digestion, and, to make things equal, the hydrochloric acid was added to the second preparation so as to complete the artificial gastric juice. For the third preparation the manufacturers claim all the ingredients necessary.

To resume. The beakers and contents were placed in a water bath at about 100° F. and kept there for three hours. In less than an hour disintegration began in A, no change occurring in B or C. In two hours, the contents of A were small masses of egg-albumen floating in solution; no change in B or C. In three hours the contents of A were in a flocculent state, and still there was no change in B or C. Further experiments showed that casein would be precipitated almost immediately by the first preparation, and, if allowed to further act, would continue the digestion, while the other two were inert.

At my request the before-mentioned firm kindly forwarded preparations of Taka Diastase and pancreatic extract for class demonstrations. A grain or two of the Diastase, introduced into a test tube containing boiled starch, liquefied it almost immediately, and after placing the tube in a water bath at 100° for half an hour, glucose was detected both by Fehling's and the picric acid tests, which showed remarkably rapid action.

For demonstrating the action of the pancreatic extract on albumen, three beakers were taken and labeled. In A was placed a piece of coagulated egg-albumen weighing two drachms, with a solution of the extract; in B, a like weight with an identical solution, to which was added hydrochloric acid; in C, the same weight of albumen and quantity of solution, to which, however, was added carbonate of sodium. All were placed in the water bath at 100° F. In two hours, the eroding action of the trypsin could be plainly seen in C, but of course none was visible in A or B. The biuret test showed the presence of peptones in C and absence of them in A and B.

Emulsification of oil was tried, and although partially successful, the lack of time and suitable conditions prevented satisfactory results.

And now, in closing this short demonstration, allow me to ask you to make a clear diagnosis before advising the digestive ferment; be sure, too, that you have the form of indigestion firmly fixed, and last of all, distinguish between the albuminous, amylaceous and fatty forms of intestinal indigestion.

Our method of prescribing for indigestion is one of the double-barreled, handed down by our medical forefathers, who scarcely comprehended what it was. In the broad glare that latter-day investigations have thrown upon the subject, we have no excuse for administering the olla podrida that, with them, was science.

1212 East Broad Street.

MODERN MEDICAL EDUCATION.

ADDRESS OF THE PRESIDENT.

J. C. WILSON, M.D., Professor of the Practice of Medicine and of Clinica Medicine in the Jefferson Medical College, etc., etc.

Fellows of the Academy:—I thank you for the honor you conferred upon me at your meeting in Atlanta in choosing me to preside over your deliberations in Philadelphia at this, your twenty-second session. To be elected President of the American Academy of Medicine is, under all circumstances, a most distinguished appointment, but coming as it did to me, unsought and unthought of, at a time when the wide usefulness of our most unselfish organization has become apparent to the whole profession of the land, at an epoch in its history when many who have held aloof from its struggles during its years of growth, are entering its ranks in the day of power, is a compliment of which I may, without unbecoming elation, be justly proud.

Your brief annual meeting affords me the pleasure of a direct personal relationship with you of only short duration, but the duties of the office have occupied my attention during the year, and have impressed upon me with increasing force the great usefulness of the work already accomplished, and the far greater importance of that which remains for you to do. Nor do I discern any end to the field which the founders have laid out for us to do till, seeing that new problems in medical education, medical ethics, and medical sociology will continue to arise, and that growing success will bring to us widening opportunities, new difficulties and responsibilities at once more arduous and more important.

Since the first meeting of the Academy in this city, September 6, 1876, remarkable changes have come to pass. The scope of medicine itself, the character of medical teaching, the requirements for graduation have, in the short period of the minority of a native-born citizen as fixed by law, undergone an evolution little anticipated by the handful of earnest men who, under the leadership of Traill Green, of Easton, then banded themselves together to organize an association whose objects are:

1. "To bring those who are alumni of classical, scientific, and medical schools into closer relations with each other.

2. "To encourage young men to pursue regular courses of study in classical and scientific institutions before entering upon the study of medicine.

3. "To extend the bounds of medical science, to elevate the profession, to relieve human suffering, and to prevent disease."

In the interval since the date of that remarkable meeting, the science of bacteriology, then viewed askance by the leading teachers and practitioners as something new and strange, has become a dominant influence in medicine. It has revolutionized pathology, it has recreated the art of healing in every department, and has given us a new surgery, in whose footsteps, with urgent stride, follows a new medicine of not less brilliant promise. It has enabled the gynecologist to realize and surpass the brightest hopes of McDowell, Marion Sims, and the Atlees. It has rescued hygiene from empiricism and placed it upon a sure basis of scientific facts. It has elevated the specialties above the narrowness of circumscribed work, and shown the specialists that for the practice of their art much more is needed than an easily acquired operative technique and a little manual and intellectual dexterity. It has demonstrated the absurdity of much of our traditional drugging and given us new systems of healing, fruitful with present achievement, and pregnant with promise of greater things, while it has indirectly revived and defined the scope of measures of treatment, such as diet, baths, and climate, for which we do not send to the apothecary. It has warned the investigator in the laboratory that the study of the causes of diseases which are ever ready, sure, swift, and certain of effect, is to-day a much more promising field of research than the too curious analysis of the special qualities of uncertain and variable drugs. In point of fact, bacteriology, the youngest of the medical sciences, scarcely known as a science to those who organized this body, has swiftly pressed to the front and now controls almost every department of the complex organization of the medical sciences.

While these changes have been going on, other developments of great importance have been taking place. Histology has assumed a definite place in the curriculum, and anatomy can no longer be studied without the microscope. The teachings of physiology, yearly more definite and precise, bear with every advance a closer relation to the requirements of practice, so that no alert practitioner feels secure unless he can read his latest treatise upon physiology carefully through at least once a year. Physiologic chemistry has made corresponding advances. Embryology must be taught as indicating the structural bases of forms and the primary relations of tissues, while comparative anatomy is essential to a complete knowledge of the anatomy of the human subject. Some knowledge of medical jurisprudence was at one time thought necessary for the expert, but to-day every practising physician must be informed of the mode of procedure in judicial inquiries, since he may at any moment be called upon to act for others and himself in a case involving questions of mental alienation, testimentary capacity, the responsibility for injuries by accident, or homicide.

These changes in the scope of medicine have not taken place without corresponding and far-reaching changes in the organization of the medical profession, and its relations to the public at large. Very often these changes have been like the growth of the knowledge to which they owe their origin, gradual and almost imperceptible, but very often they have been abrupt and violent. Progress means readjustment and this cannot take place without occasional perturbation and commotion—a fortunate circumstance since it enables the too conservative to become aware of the fact that something is going on. I may remark in passing that constant readjustment and occasional violent reaction are inherent to intellectual advance, and when they cease to disturb our quietude, stagnation ensues.

In the medical life of the last quarter of a century, especially in this country, the perturbations and violent readjustments of advance have been principally and most keenly felt in the teaching institutions, from some of which the advances have originally in every instance directly or indirectly proceeded.

The shock has usually been violent and the adjustment tardy, a fact for which we find an adequate explanation, first in the conservatism of scholastic institutions, secondly, in the tenacity with which men cling to customs which they have come to look upon as vested rights.

Tardy as they have been, however, the successive adjustments of the colleges to the advances of knowledge have been complete, each in its turn, and as new adjustments take place they will be as nearly as possible adequate to the essential requirements of particular times and circumstances. This prediction rests not only upon the experience of the past, but upon the fact that while the knowledge upon which medicine rests is scientific, the application of that knowledge is an art, and subject to the conditions by which art in all its manifestations is influenced, not the least of which is competition.

Here we observe a fine example of action and reaction—increasing knowledge calling for better teaching, and better teaching in its turn tending to the advance of knowledge.

The character of the teaching has, in fact, during the period referred to, undergone changes scarcely less radical than the subject-matter to be taught.

It has become less didactic, and more demonstrative; less rhetorical, and more explicit; less showy, but more convincing. The seven learned professors and single demonstrator that constituted the faculty of a well-equipped school in that day, presently found the growing task too much for them. They had not time to do the work, and worse still, they realized ere long that their combined knowledge was unequal to the undertaking. They did not know enough. So it came to pass that with each annual announcement new subjects were placed in the curriculum and bright young fellows, or a trained specialist or two were taken on as teachers. And this went on, until to-day the teaching force of a modern medical college of the first rank, including professors, assistants, lecturers, demonstrators, and the members of the staff of the attached hospital and dispensary service is to be counted by the score. Indeed, malice has observed of more than one such institution that the occupants of the chairs outnumbered the occupants of the benches.

As the old faculties had not time to do the increased teaching, so the student found his time too short and the time-element became a factor in the process of evolution. First, the term was lengthened a little, then more. This proving insufficient, a year was added to the course, and later another year. Now, instead of two annual sessions of scarcely five months each, the student of medicine must attend four annual sessions of seven months or more. The number of sessions is now determined in several of the States by law, but the public opinion that has made itself manifest by legal enactment in some places, is growing in power all over the land, and the time is not far distant when the two and three years' schools will cease to exist because men will be ashamed of their diplomas. Such schools, it is true, have thrived for a season, but their prosperity has been short-lived, first, because it arose from a shifty expediency, and secondly, because the sober after-thought of the American people is right. The progressive schools have sometimes had poor classes during the periods of transition, but the change has, in almost every instance, been followed by a notable increase in the number of students.

The requirements for graduation have undergone progressive changes not less important. The examinations have long since ceased to be perfunctory. They are now tests and, in most instances, severe tests of the knowledge of the applicant for the degree in the fundamental branches of medical science, and not rarely in the application of his knowledge at the bedside in the diagnosis of an actual case and in formulating a scheme of treatment. They are sometimes oral only, frequently written, and occasionally both written and oral. In the written examinations the use of improper aids is frequently, but not always prevented by obvious and unfortunately necessary measures. In the clinical branches, the possibility of extraneous help or collusion is reduced to a minimum by the character of the questions. Twenty years ago the hope that the honor system would ever obtain in examinations for the doctorate in medicine would have seemed chimerical; to-day there are sane teachers who confidently expect its arrival. Surely the standard of honor ought to be as high in medical as in military life. In many schools the final averages in particular branches are made up of the results of several examinations conducted by different teachers—the professor, a demonstrator, and the lecturer in the special department, each counting a prearranged percentage of the whole. The examinations no longer being a mere form and their subject-matter being purely technical, no student, however clever, who has neglected his work or spent his time in riotous living expects to pass by chance—to "skin through" in the words of an earlier time. Indeed, it not infrequently happens that students who have among their classmates the reputation of being fairly well prepared, absent themselves from the examination in one or two branches at the last moment, and defer graduation for another year rather than take the risk of being actually plucked.

The development of medical education in the twenty-one years which measure the life of this Academy, in the scope of the knowledge to be acquired, in the character of the instruction, and in the requirements for graduation, constitutes a remarkable epoch in the history of medicine. Disregarding the post-graduate schools, the aim and scope of which are not immediately considered in this argument, the changes have been so great as to have brought into full bloom a flower conspicuous in the brilliant intellectual development of the end of our century—the higher medical education. It is not assuming too much, Fellows of the Academy, to assert that this body has played an important, though inconspicuous part in bringing about these changes, fraught with so much importance to the well-being of the profession and to society at large. It would be strange, indeed, were this not the case. The very purpose of our organization was to promote such interests. The papers that have been read, the discussions that have followed, the enthusiasm that has been aroused, the force of association, the weight of opinion developed and fostered by the contact of men interested in the promotion of the highest professional aims have, during these years, exerted a combined influence upon the profession which has been potent for good. I congratulate you that during the existence of this body many of its highest aims have been achieved.

A word in passing in regard to examining boards and the license to practise. It is a happy commentary upon the purity of professional aims that the most advanced medical educational institutions have been the most prompt to make sacrifices to the common good. One after another, in State after State, the colleges have relinquished a fundamental and most important charter right; namely, that the diploma should confer upon its recipient the right to practise medicine. The movement for separate legal requirements, first registration, then State examination, had its origin in those interested in the higher medical education, in many instances in the faculties themselves of the most advanced and well-equipped schools. This movement, it is true, was aimed primarily against quacks of the itinerant kind and the diploma mills, but in its more recent development it has dealt serious blows at the inadequately equipped and officered second and third rate legally chartered colleges. It is certainly a remarkable fact that educational institutions should, in the best interests of a common profession, voluntarily relinquish the most important franchise conferred upon them by charter. What the effect of the present tendency toward reciprocity among the States in the granting of licenses and the system of inter-State indorsement may have upon certain of the schools remains to be seen. Greater uniformity of facilities for instruction will become necessary. Geographical situation and local conditions in some instances seriously hamper, in others render absolutely useless efforts to supply adequate clinical instruction. There are schools in which the elementary teaching is of the best character, that cannot secure the facilities for clinical work. The lengthening of the term from three to four years cannot, under these circumstances, meet the requirements of the case, but if a general comity in regard to the license to practise is to be established by the boards of different States, the law must regulate the length of the time of instruction and all boards must equally require that applicants for the license should have attended a four years' course. In that case students will naturally repair to those schools in which the fourth year is occupied in clinical work. This would constitute in many instances a serious misfortune, first, because of the unwieldiness of enormous classes, and secondly, because the non-clinical instruction in small college towns may be, and often is, of the highest character. I would suggest that some of the difficulties may be overcome if students presenting certificates of having successfully passed their third year examinations should be admitted to the schools having large clinical facilities without examination for the fourth year, and be permitted to return to the college from which they came for graduation. This would, it is true, be a makeshift, but it would tend to equalize the opportunities of the students in different sections of the country and thus render feasible an inter-State reciprocity among the licensing boards.

Equally important is the movement toward uniformity of requirement for admission to the medical schools. It is at this point that the barrier must be set up by which men unfitted for professional life, either by lack of capacity or inadequate early education, are to be saved the waste of time and disappointment attending failure to successfully pass the examinations either in course or final. So important has this matter appeared to those interested in medical education that the degree of Bachelor of Arts or its equivalent has already been made a preliminary requirement in one medical school and is announced to go into effect in two others. The importance of this measure being granted, serious discussion has arisen as to how the prolonged course of four years in a collegiate department and four years in the medical school can be avoided. The president of one of our great universities has gone so far as to advocate the shortening of the collegiate course to three years. By another plan the student in the academic department is permitted to present himself for the final examination at the end of the third or junior year. If successful, he then receives a certificate to that effect. This certificate entitles him to begin the course in medicine, the degree of Bachelor of Arts in the academic department being conferred upon him with his class at the end of the ensuing year. Should such a degree as a preliminary requirement be generally adopted by the medical schools, some expedient by which the academic course can be shortened a year will be a necessity, since the technical medical course cannot be shortened even in the case of students who do biological work as electives, without detriment to their medical education. Eight years of study prior to obtaining the doctorate in medicine and the license to practise, constitutes a longer period of time and involves a greater expense than the majority of aspirants for honors in the medical profession can afford. This fact has added force when we consider the time spent by many graduates in serving as hospital internes, and in other work for which they receive no pecuniary recompense.

We, as a working society, have seen not only the wonderful advance in medical education of which I have spoken, but we have also seen the organization of the College Association, one of the objects of which is to make that advance uniform and permanent. We have witnessed not only the enactment of laws by which in many States the license to practise can only be obtained by successful examination under rigorously defined regulations, but also the Confederation of the State Boards of Examiners for the purpose of securing the highest degree of efficiency. In short, in medical education our eyes have looked upon a burst of light like a new dawn, and it has been ours to have had some part in preparing for the day. But there is more to be done, and we may count ourselves happy that we are to do much of it. For what we do as an organization we feel sure will be done well. The educational problems are far from being yet solved; the relations of the profession to the State are not yet well defined; questions as to expert testimony are still vexed; the status of medical men in armies and navies is not satisfactory; hospital and dispensary abuse is a crying evil; the growth of the commercial spirit is a dry rot within our ranks. Here are problems for us to work out—problems of great moment to us as members of the noblest of professions. Individuals may toil at them and do good, but if we as an association make them the objects of our united labors, we will make ourselves felt as no single man or many single men working apart can ever, no matter how great their individual gifts and energy, succeed in doing.—Bulletin of the American Academy of Medicine.

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SELECTIONS.

ACUTE DILATATION OF THE STOMACH.

By HENRY WALD BETTMANN, M.D., Cincinnati,
Pathologist of the Cincinnati Hospital; Attending Physician of the Jewish Hospital, etc.

On the subject of acute dilatation of the stomach much confusion exists among medical writers. Most of the text-books dismiss the subject with a few brief lines; others deny the existence of the affection. No textbook with which I am familiar gives more than a mere passing notice of the disease. So little that is definite has been written on the subject that the term "acute dilatation" has come to signify different conditions to different authors. Boas describes under the heading "Acute Dilatation of the Stomach"[1] a case in which the stomach of a normal young man showed marked evidences of dilatation three weeks after an indiscretion in diet. The same author elsewhere alludes to cases of acute dilatation that follow the excessive ingestion of food and drink. As early as 1867 Erdmann[2] related a case of paralysis of the walls of the stomach following a fall. The patient had symptoms of a distended stomach for seven days following the accident, but the stomach gradually regained its contractile power. The only other symptom was pain after taking food, which was allowed in very small quantities.

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[1] BOAS: "Ein Fall v. Acuter Magendilatation," Deut. Med. Woch., 1894, p. 172.

[2] ERDMANN: "Ein Fall v. Akuter Magenerweiterung," Virch. Arch., vol. 43, p. 295.

It is thus evident that conditions which have little in common have been classified under a common name, much to the confusion of the subject. The English writers have given the title "Acute Dilatation of the Stomach" to still another group of symptoms, and quite a series of similar cases have been reported by them. It is especially to this latter group that I wish to call attention, and I think that the following facts will prove (1) that there is such an affection as acute dilatation of the stomach; (2) that the disease is fairly uniform in its onset, course, and tendency to a rapidly fatal termination; (3) that the treatment must be prompt and active, and that some lives may be saved by early recognition of the condition and energetic treatment.

The following case was reported by Miller and Humby[1] in 1853:

Mrs. M., aged forty-eight, who usually enjoyed good health, was seized on the night of March 8, 1853, with a sudden vomiting spell. Her condition indicated marked collapse; her pulse was faint and only 40 a minute. She vomited large quantities of fluid, filling five basins in the course of the night. She vomited steadily for the three days following; her abdomen was sunk and depressed, her tongue dry, the feces lumpy. On the 10th of March her condition grew much worse; the vomiting ceased, the abdomen was distended and yielded fluctuation to palpation. Then followed a few days of delirium; the weakness increased, and on March 17 she died. The post-mortem examination revealed an enormously distended stomach, filling the whole abdomen. All the other viscera were healthy. An illustration of the distended stomach is printed in the Transactions.

A very similar case, occurring in 1856, was described by Hughes Bennett.[2]

His patient, a young man aged twenty-six years, had severe abdominal symptoms for three days. Violent vomiting occurred on the fourth day, becoming uncontrollable on the seventh day; on the ninth day the patient died. The only pathological condition found on the post-mortem table was an enormously distended stomach.

The attention of practitioners was directed to the subject of acute dilatation of the stomach by Hilton Fagge,[3] who reported two striking cases in 1873.

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[1] Trans. of the Path. Soc. of London, 1853, vol. iv, pp. 137-139.

[2] HUGHES BENNETT: "Principles and Practice of Medicine."

[3] C. HILTON FAGGE: "On Acute Dilatation of the Stomach," Guy's Hosp. Reports, vol. xviii, third series, 1873, pp. 1-22.

The first case was thus summarized by Fagge: Acute dilatation of the stomach, with paralysis of its muscular coat supervening upon a retroperitoneal abscess communicating with the duodenum; use of the stomach pump; evacuation of seven pints of fluid; return of the stomach to its natural dimensions and seat; death from exhaustion. The post-mortem examination showed that the pylorus was unobstructed; there was no trace of peritonitis; about one-half pint of greenish fluid was found in the stomach, which was of a normal size.

The second case, which is far more interesting and instructive, occurred under the care of Dr. Rees, in 1870. The patient, a male aged thirty years, was in the wards of Guy's Hospital, taking ol. morrhuæ as a tonic. Very suddenly, on December 15, 1870, he began to vomit; palpation of the abdomen elicited a loud splash. Urine was secreted in very small quantities, being removed with the catheter. On the third day he had two stools after an enema. The vomiting continued unabated, the vomited matter being a greenish fluid. The temperature became subnormal, the pulse very rapid, and on the evening of the third day the patient died of exhaustion. When the abdomen was opened on the post-mortem table the stomach was found to fill the whole abdomen, the intestines being crowded into the pelvis. The liver, intestines, kidneys, and heart were healthy. During its removal from the body the stomach was wounded. Thin greenish fluid escaped, and the stomach returned to its normal size.

C. Hilton Fagge, from these two cases and from the others previously reported, made the following conclusions:

1. Acute dilatation of the stomach may arise in young subjects in whom the organ has apparently been healthy. The actual process of enlargement is probably more or less gradual. At first it produces no symptoms, but when these occur they are sudden in onset and very violent, and may lead to death in a few days.

2. Towards the end the abdomen is unsymmetrically distended, the left hypochondrium is full, the right hypochondrium is flattened; the course of the lesser curvature can be followed from the left hypochondrium downward and to the right. There is fluctuation and splashing in the hypogastrium. The prevailing percussion-note over the tumor is tympanitic.

3. The symptoms are those of collapse. The vomiting at first is persistent; several quarts of fluid may be evacuated; then vomiting may cease entirely, the stomach becoming over-distended (compare the urinary bladder). The urine is scanty; no absolute constipation exists.

4. The necropsy reveals a stomach enormously distended. The stomach may return to its normal size after removal, showing no graver lesions than slight lacerations of the muscular coat (compare the striæ gravidarum).

5. The stomach pump may save the life of the patient. After its use the stomach may regain its normal size.

Notwithstanding the reputation of Fagge and the apparent conclusiveness of these cases, the existence of the disease in question was doubted by most writers at home and abroad.

In 1883, Henry Morris[1] reported a new case which is strikingly similar to the second one reported by Fagge. The patient, a valet, aged thirty-seven, was operated on November 11, 1882, for a suppurating ankle joint. One hour after the operation he began to vomit a fluid in large quantities, without pain or effort. The vomit was a thin, greenish fluid, unmixed with food, neither frothy nor offensive, and came up in large gulps, being evacuated with considerable force. The vomiting continued without cessation for forty hours, at least ten pints having been ejected. During this period the patient was collapsed, his temperature subnormal, his face pinched, the pulse very rapid. Notwithtstanding the nutrient enemata containing brandy, he died of exhaustion. The post-mortem examination revealed a very distended stomach, which almost completely filled the abdomen, and contained twenty-eight ounces of thick, grumous fluid. There was no obstruction of the bowels, no peritonitis.

In 1887 Dr. Dyson reported a similar case before the Sheffield Medico Chirurgical Society.[2] The patient, a pregnant woman, aged forty-one, took a glass of cold milk immediately after some exhausting work. Soon afterwards she began to vomit large quantities of bile-stained fluid. In thirty-six hours she miscarried a seven-months-old fœtus. The vomiting continued. All the physical signs, as described by Fagge, were present. On the fourth day she died of exhaustion, vomiting having continued until the end. No post-mortem examination was made.

Another conclusive case, with necropsy, was reported by Schultz.[3] A farmer's daughter, aged twenty-four, who had previously been strong and healthy, was taken sick in August, 1889, with a vague fever which resisted treatment and reduced her to a condition of extreme emaciation. On November 28 she was admitted to the hospital: her temperature was normal, her pulse 84, but marasmus was extreme. With careful nursing and feeding she improved steadily. After two weeks she took food with relish, had no pains, no nausea. On December 17 she complained of intense abdominal pain, and began to vomit: all the next day she vomited a bile-stained fluid and had intense abdominal pain. The abdomen began to be distended on the 19th; the peristaltic movements of the dilated stomach were visible through the abdominal wall. On that day she had a normal stool. Her vomiting was incessant during the night of the 19th, but ceased on the 20th, when distention became extreme, and the patient died of exhaustion. The necropsy showed an enormously distended stomach, with thin walls. There was no stenosis of the pylorus, the duodenum also being dilated.

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[1] Henry Morris: "The Stomach from a Case of Acute Gastrorrhea or Acute Dilatation of the Stomach; Death Within Forty Hours of the Onset of Vomiting," Trans. of the Path. Soc., London, 1883, vol. xxxiv, pp. 82-88.

[2] Dyson : Lancet, 1887, p. 883.

[3] Schultz: "ZurCasuistik d. Acuten Magenerweiterung," Jahrbuch d. Hamburger Staatskr. Anst., 1890, vol. 2, p. 145.

In 1891 Dr. D. W. C. Hood[1] reported the following case: A servant girl, aged nineteen, was admitted to the West London Hospital, December 24, 1890, suffering from a gumboil; three days later the pus was discharged into the mouth, and she was apparently convalescent; on December 28 she developed pneumonia at the base of the right lung; on the 29th she began to vomit a thin, greenish fluid; later in the day the fluid became inky, and at 7:45 P. M. she died. The necropsy disclosed a very dilated stomach, which contained two quarts of fluid. The duodenum and upper part of the ileum were also distended. Both lungs were engorged, and the pericardium was the seat of a purulent inflammation. The stomach walls were so thin that at places they looked like writing paper.

At a meeting of the Bournemouth Medical Society in 1895, Mahomed read the notes of a necropsy on a case of acute dilatation of the stomach. The affection arose during convalescence from pleuro-pneumonia, and was rapidly fatal. The patient was a volunteer, twenty years of age. Mahomed concluded that, in addition to the loss of muscular power, excessive secretion from the gastric mucosa is an important factor in cases of acute dilatation.

Another fatal case was reported in 1896 by Albu.[2] The patient, aged twenty-six, a stonemason by trade, was taken sick with a severe attack of scarlatina on May 20, 1895. Convalescence was fully established after the first week. On May 31 the patient, without having indulged in any indiscretion of diet, complained of severe epigastric pain, and began to vomit violently. For the first twenty hours vomiting was incessant; at first food was ejected, but later only a thin, greenish fluid. The pulse was small, the intellect clouded. On the afternoon of the second day the abdomen became distended; clapôtement could be elicited; the vomiting ceased. Gastric lavage was practised, but without benefit. The apathy increased. Toward evening the patient became delirious, and died during the night. Albu calls special attention to the existence of two symptoms which mark the last stage of this peculiar disease—the absence of visible peristaltic movements and the cessation of vomiting. These symptoms can be referred to paralysis of the muscular walls of the stomach. The necropsy disclosed the usual condition—an enormously distended stomach containing a greenish fluid. There were no ulcers, scars, nor obstructions at the pylorus. The kidneys showed signs of interstitial inflammation; myocarditis was present. The only possible cause of death was the distended stomach. Albu thinks the disease is a central paralysis due to the toxins of an infectious disease.

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[1] D. W. C. Hood: "A Case of Acute Dilatation of the Stomach," Lancet, 1891, ii, p. 1389.

[2] Alb. Albu: "Ueber Acute Tötliche Magendilatation," Deut. med. Woch., February 13, 1896, p. 102.

In the winter of 1895 I had the opportunity of observing a case of acute dilatation of the stomach which, in every respect except the outcome, resembles the cases cited above. I read a description of the case at a meeting of the Academy of Medicine in January, 1897, but as the case has never been published, I append the description as read:

A case of acute dilatation of the stomach during convalescence from typhoid fever; lavage; recovery.

Bessie Lampe, aged seventeen, of Russian extraction, very well developed and nourished, was admitted to my service in the Jewish Hospital October 23, 1895. She had enjoyed the best of health up to two weeks prior to admission, when she began to complain of headache, anorexia, fever, lassitude, and vomiting. She was treated by Dr. Bauer, who made the diagnosis of typhoid fever, and placed her under appropriate treatment. Upon admission to the hospital she presented the typical picture of typhoid fever; her intellect was clouded, her spleen enlarged, her fever ranged between 101° F. and 103° F. She had diarrhœa; the stools were typical of typhoid fever, and in a few days the eruption appeared over the abdomen. The only serious complications were vomiting, which was obstinate and occurred one or more times daily, and retention of urine, which necessitated catheterization of the bladder throughout the whole course of her sickness. The diagnosis of typhoid fever with acute gastritis was made.

From the 23d of October to the 7th of November the disease progressed naturally; the patient was delirious at times, and vomited food frequently, and complained considerably of abdominal tenderness, but the fever gradually declined.

From the 7th of October to the 13th convalescence was fairly established; she felt very hungry, took nourishment (which consisted of ice cream, tea, soft boiled eggs, and chicken broth) well, and slept nine to ten hours every night under the influence of trional.

On the 13th of October she vomited once, but felt well.

On the 14th she complained of severe abdominal pain, became very nervous, being delirious at times, and vomited eight times. Food was stopped. The abdomen was enlarged and tender.

On the 15th the restlessness continued until 10:30 P. M., when she suddenly collapsed. Her temperature fell to 96.5° F.; she was pulseless at the wrist.

It was with the greatest difficulty that she was kept alive during all the next day, and three nurses were constantly busy. All day she was pulseless and cold, and vomited large quantities of a thin, greenish fluid. Now and then she would swallow a small quantity of cracked ice with a few drops of chloroform, but for the most part her sensorium was dulled. Her bowels moved spontaneously, the stool being brown and watery. About 700 Cc. of urine were drawn during the twenty-four hours. During the day she vomited twenty to twenty-five times, the quantity of fluid varying from 25 to 150 Cc. each time. The total quantity must have been nearly three pints.

On the 17th the vomiting continued unabated, the fluid, of light green color, coming up in large quantities. Her bowels moved three times spontaneously. The abdomen was still distended. The condition of collapse continued, though the patient was conscious throughout the day.

On the 18th the condition of the patient was so desperate that gastric lavage was determined upon. A very thin tube was passed without much difficulty, and a large quantity of thin, greenish fluid was removed. The condition of the patient improved almost immediately, the abdominal distention disappeared, the sensorium became clearer. Rectal alimentation was resorted to. The vomiting ceased for about eighteen hours. The bowels moved spontaneously, the stools being thin and brown.

Gastric lavage was practiced once or twice daily for three days, and the patient was nourished entirely by enemata for five days. Her convalescence was slow. She vomited at times and remained weak, being slightly delirious now and then.

On the 21st she retained some iced tea and orange juice.

From the 22d on her improvement was more rapid, her stomach retained koumyss, buttermilk, chicken broth, and her strength slowly returned. She remained in the hospital two months more, suffering from ulcers on the legs due to sloughing in places where ether had been injected. During the past year her health has been perfect, and last week she presented a picture of robust strength.

I think there can be no question as to the diagnosis in this case. The very violent and incessant vomiting, the condition of collapse, the distension of the abdomen, the nature of the vomited material, are all very characteristic. The disappearance of abdominal distension after lavage confirmed the diagnosis. The regular spontaneous movements of the bowels for several days, and the persistence of liver dulness precluded the idea of a perforation, which was the first suspicion.

Brinton[1] refers to similar cases occurring during convalescence from typhoid and typhus fever. He says: "It occasionally happens that a patient recovering from fever is suddenly seized with intense pain in the belly, which becomes somewhat tympanitic and perhaps tender to pressure, though not otherwise suggestive of peritonitis. But even before that brief lapse of time which would often be requisite fully to develop local symptoms of the latter disease, he either dies of extreme prostration, or, as in most cases (in all I have seen), recovers with extraordinary rapidity."

All the cases hitherto recorded have ended fatally except one, to which, unfortunately, I have no access. The title of the paper, however, gives an outline of the disease, viz., "Un cas de dilatation de l'estomac à début brusque chez un enfant de sept aus; guerison rapide par le lavage de l'estomac."[2]

I have tried to report above, all the cases of this form of acute dilatation of the stomach found in the literature.[3] A study if the cases cited must convince any one that a distinct disease, which cannot be called by any other name than "Acute Dilatation of the Stomach," or "Acute Gastrorrhea," does exist. The great rarity of the affection has caused it to be slighted by authors, and even by the specialists for diseases of the stomach.

Boas,[4] in the 1893 edition of his work on "Diseases of the Stomach," makes no mention of acute dilatation. Sidney Martin,[5] in his chapter on dilatation simply speaks of the "so-called acute dilatation," and refers vaguely to Fagge.

Very recently Pepper and Stengel[6] have given a very exhaustive exposition of the subject of dilatation. They pass loosely over the subject of acute dilatation, however, jumbling together very ill-fitting material in the following sentence (p. 38): "In rare cases, as in those of Endman (Erdmann?), Oser, Kundrat, and Boas, atonic dilatation may be acute, and in such instances is prone to be serious." This is certainly a wholly inadequate presentation of the subject.

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[1] WM. BRINTON: "Lectures on the Diseases of the Stomach," Phila., 1865, p. 245.

[2] SIMON: Journ. de clin. et de terapie infant, 1894, ii, p. 76.

[3] Several cases are referred to in the literature, but I have been unable to get the original reports. These are:

KELYNACK: "Notes from the Post-Mortem Room: A Case of Acute Dilatation of the Stomach," Med. Chronicle, Manchester, 1892, pp. 123-6.

MONGOUR: Arch. clin. d'Bordeaux, Nov. 1894.

HAYEM: La Medicine Moderne, July 13, 1895 (erroneously stated by Sajous' Annual, 1896, to have been reprinted in the British Med. Journal, Sept. 7, 1895).

Other cases have been reported under the title "Acute Dilatation of the Stomach," but do not belong to the class of cases grouped under that name above. Veeder (British Med. Journal, 1894, vol. i, p. 240) describes a case of sudden death after a hearty meal. The woman had had dyspeptic troubles for years. Todd (British Med. Journal, 1894, vol. i, p. 522) describes under the same heading a case which was undoubtedly one of chronic dilatation with sudden severe symptoms, relieved by lavage. Albu speaks of cases by Köberle, Gross, Hunter and Fränkel, but gives no reference of any kind.

[4] BOAS: "Diagnostik u. Therapie d. Magen-krankheiten," Leipzig, 1893, vol. ii.

[5] SIDNEY MARTIN: "Diseases of the Stomach," Phila., 1895.

[6] PEPPER and STENGEL: "Dilatation of the Stomach," American Journal Med. Sciences, January, 1897.

Ewald[1] says (p. 137): "The symptoms of dilation of the stomach always develop slowly." And later (p. 145) he says, in speaking of the course of chronic dilatation: "But some day this compensation fails, and then suddenly, or in a surprisingly short time, all the symptoms of dilatation appear. These are the cases in which the dilatation arises acutely, and which are spoken of especially in English literature (Fagge)."

This opinion of Ewald, ill-supported as it is by the evidence at hand, has found general acceptance and is echoed by late writers.[2] Ewald denies the actual occurrence of acute dilatation, but thinks that the group of symptoms called by that name is simply an episode in the history of chronic dilatation. Though it is impossible to affirm that this may not have been the case in some of the reported cases, it is certain that it could not have been so in some of them. It is well known that enlargement of the stomach may be latent, and may not manifest itself by symptoms, and this fact lends color to the view of Ewald.

In most of the reported cases the patients had enjoyed good health up to the time of attack. In some of them the patients had been under observation for some time, and manifested no symptoms referable to the stomach. But, leaving all conjecture aside, the two cases reported by Fagge are proof conclusive that we are dealing with an acute condition. In the first case Fagge reports the stomach as returning to its normal size after lavage, seven pints of fluid having been withdrawn. At the necropsy the stomach was found to be of normal size. The withdrawal of seven pints of fluid during life shows that the stomach must have been dilated; the return of the stomach to its normal size proves that the dilatation must have been acute, the tonicity of the walls having been only temporarily disturbed. The second case reported by Fagge is no less conclusive. At the necropsy the stomach was found to fill the whole abdomen. During its removal it was wounded; its contents escaped slowly, and it returned to its normal size. What could be more convincing that the dilatation had been acute?

Other writers describe slight lacerations in the muscular wall of the stomach, and this is additional evidence (if such be needed) that the process is acute.

Especial stress should be laid on one other feature of this disease, viz., the immense quantity of fluid which is ejected or which is found in the stomach after death. This quantity is often incredibly large, and greatly exceeds the amount ingested. It is certain that the green color is due to admixture with bile; the fluid must in part be secreted by the stomach, and excessive secretion is certainly an important factor in producing the symptoms. This is especially noted by Henry Morris,[1] who suggested the name "Acute Gastrorrhea." Mahomed also called especial attention to this feature of the disease.

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[1] EWALD: "Diseases of the Stomach," English translation by Manges (1895).

[2] C. F. FLEISCHER: "Specielle Path u. Ther. d. Magen u. Darmkrankheiten," Wiesbaden, 1896, p. 886.

Summing up, the following facts may be postulated:

1. During the course of or convalescence from some acute or chronic disease the stomach may undergo rapid dilatation.

2. This condition is marked clinically by a sudden and violent onset; vomiting is violent and intractable; large quantities of fluid are ejected; the fluid is usually greenish, due to admixture of bile. The patient is reduced to a state of collapse or exhaustion, which may prove fatal in a few days.

3. During the progress of the disease the abdomen becomes distended, the right hypochondrium remaining flattened. The bowels move spontaneously, and a splash may be elicited over the site of the distension. The mind usually becomes clouded.

4. If treatment is unsuccessful the distension of the abdomen increases, vomiting ceases, and the patient dies of exhaustion.

5. The indications for treatment are, first, supportive measures; and, second, the use of the stomach tube one or more times as early in the case as possible. Rectal feeding should be resorted to, and nothing should be allowed "by the mouth" until the vomiting has been nearly or quite controlled.

6. Treatment without lavage is unavailing, and the use of narcotics worse than useless.—The Cincinnati Lancet-Clinic.

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WHEN TO CALL A SURGEON IN APPENDICITIS.*

By GEORGE W. GAY, A.M., M.D.,

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The remarks which I shall present to you this evening are intended especially for the family physician, who does not operate himself, and who does not wish to assume the sole responsibility of deciding those important questions peculiar to appendicitis. It falls to the lot of the general practitioner in the great majority of instances to see the victims of this malady in its early stages, and it is scarcely too much to say, that not infrequently the result is largely determined by the measures adopted at that time. It is not worth while to enter upon a discussion as to whether appendicitis is a surgical or medical affection. Suffice it to say, that the general practitioner from the very nature of his relation to the public, is obliged to meet, to accept, and to treat these patients during the earlier part of the attack according to the best of his ability, and with the resources at his command. For these reasons any suggestions or information which the surgeon can furnish from his standpoint renders the physician more efficient, and better enabled to cope with one of the most formidable maladies of the present day.

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[1] Loc. cit.

* A lecture delivered in the Graduates' Course of the Harvard Medical School, November 5, 1896.

Ever since Dr. R. H. Fitz presented his justly celebrated paper to the profession, now more than ten years ago, the subject of appendicitis has occupied a foremost rank in the medical societies and in the medical literature of this country. Physicians and surgeons have vied with each other in studying and in reporting cases, with a view to deciding the many disputed points in relation to the cause of the disease, the treatment and its results. The practitioners are few who have not been called upon to treat these cases, and who have not had reason to feel that much is still to be learned before a uniform standard of treatment will be attained. The last word has not been spoken or written; there are still mooted points to be settled; and for this reason the writer felt that he could not better occupy your time and attention than by briefly indicating some of the conditions and circumstances under which the general practitioner may very properly and wisely seek the advice, the support and the aid of the practical surgeon.

Much has been learned from the family physician in the past, and will continue to be in the future, in regard to the early symptoms, the course of the disease, the results of the so-called "expectant" treatment, and many other points. While from the very nature of things there can be no hard and fast custom in these matters, yet, as a rule, these patients require both medical and surgical treatment; so they should receive the services of the surgeon in addition to those of the physician. It goes without saying, that many patients cannot command the services of an expert, but must rely upon the aid which is at hand. The heartiest co-operation should, and I believe does, exist between the physician and surgeon in the management, not only of this disease, but of all others in which the services of both may be required. The best interests of the patient, of the profession, and of the public all lie in the same direction, and the more thorough and cordial is the understanding upon these matters, the better will it be for all concerned.

It is hardly necessary for me to say that I do not belong to the radical wing of the profession, which maintains that every case of appendicitis demands operative interference. The final and conclusive test of experience would seem to prove beyond a reasonable doubt the fallacy of that plan of treatment. According to Hawkins, of 264 cases of appendicitis, 190 ran their course without suppuration. Dr. Fitz thinks "that from two-thirds to three-fourths of all cases are likely to recover from the immediate attack without surgical treatment." Dr. M. H. Richardson says that of the cases to which he is called to decide the question of operation, about half recover without it. Dr. McBurney believes that considerably more than half of the patients will recover from the first attack without operation.

In 46 consecutive cases under the reader's care at the Boston City Hospital an operation was performed 24 times, with nine deaths. Of course, only the severest cases received radical treatment. The lighter cases, 22 in number, were treated "expectantly," and all recovered.

Now, unless a fairly large proportion of all these patients who recover without an operation are again seized with this disease in the near future, it will be difficult to convince any considerable number of our fellow-practitioners that every one of these patients should have been treated by abdominal section. Many individuals in our midst, including several of our own profession, have been through one or more attacks of appendicitis in years long gone by, and have to all appearances reached a safe and permanent recovery without operation. It is difficult for the ordinary practitioner to understand why a radical operation should be universally resorted to in any affection, when about 50 per cent. of the victims will in all probability make a safe and permanent recovery without it. As human nature is at present constituted, a large proportion of our patients will very likely prefer to run their chances of the future, rather than submit to an operation, not wholly devoid of danger, for a single attack of appendicitis which is neither severe nor prolonged. I am fully aware that occasionally an apparently mild case suddenly assumes an alarming and dangerous condition, but I question if this event occurs often enough to justify operation in every instance. At all events, the experience of the past ten years has not yet taught any such lesson to a majority of our best men, who have the most to do with this malady. What the future may have in store for us on this point remains to be seen.

The ground taken by the reader is, that patients with appendicitis, who are in collapse, or who are not sick enough to go to bed, or in whom the symptoms are mild and not prolonged, or, if severe at first show a decided change for the better within forty-eight hours, do not require an immediate operation. The larger proportion of cases coming between these extremes may or may not require surgical interference, according to circumstances. It is for us to study each case carefully by all the light we can get, and try to decide judiciously as to which cases require operative treatment, and which can be safely carried through without it.

As a preliminary to our subject a few words upon the diagnosis of appendicitis may be permitted. It is pretty well settled in the minds of the profession that peritonitis in the male means appendicitis, whatever may be the peculiarity of the symptoms. The exceptions to this rule are so uncommon that our treatment need not be influenced thereby. Occasionally a volvulus, an intussusception, a band, or twist of the bowel, or a mesenteric thrombus may be found instead of a lesion of the appendix, but the symptoms will be grave, and an operation will be as certainly indicated, which of itself will, in all probability, clear up the diagnosis, as well as give the patient the best possible chance for recovery.

The most reliable and important symptoms of acute appendicitis are pain and tenderness in the abdomen. They are always present to a greater or less degree in the early stage of the affection, and the latter persists so long as the active process is going on. Of such vital importance is tenderness in the acute stage as a factor in the diagnosis, that its absence would make one hesitate to call the disease appendicitis, even were the other symptoms present to a marked degree. The only time the writer ever opened the belly for supposed appendicitis without finding it, was in the case of a middle-aged gentleman, who had suffered severely for three or four days with pain in the bowels, requiring repeated doses of opiates. The abdomen was distended and tympanitic; there was paresis of the bowels, with obstinate constipation, grayish, shrunken skin, pinched features and prostration. He was evidently a very sick man, and apparently suffering from general peritonitis. For the past few years he had had several attacks of a similar character, but of less severity. There was little or no tenderness of the abdomen. The operation revealed a healthy appendix. The intestines were markedly congested, but free from lymph or adhesions. At the autopsy nothing further was found to account for the symptoms. There was neither band, twist, intussusception or other lesion found upon a careful examination. The only symptom wanting in this case to complete the clinical picture of appendicitis was tenderness. Its absence was fatal to the diagnosis.

Pain and tenderness, then, are the principal symptoms of appendicitis, which seldom fail to be present from the beginning; and their severity and persistency, as a rule, are an index to the gravity of the attack.

Tenderness is the main feature which distinguishes appendicitis from colic or functional affections of the abdominal organs. The pain is frequently as severe in the latter as in the former, but it is not as persistent nor as prostrating in the functional as in the inflammatory affection.

In the early stages of appendicitis the pain is located in the vicinity of the navel rather than in the right inguinal region. This fact is very misleading to many physicians, who naturally expect to find it referred to the usual seat of the appendix. It is to be distinctly remembered that both the pain and the tenderness may not be located in the region of the appendix for two or three days after the commencement of the disease. If, however, the patient is very sick and steadily growing worse, be assured that these symptoms will sooner or later declare themselves in an unmistakable manner, thereby removing all doubt as to the character of the disease.

By way of summary it may be said, that the cases are rare in males and in young girls in which sudden, acute abdominal pain accompanied by tenderness on deep pressure upon the right side of the median line does not indicate a lesion of the appendix. The writer submits that this is a good working rule for every-day practice. The exceptions are so uncommon, and an early diagnosis is so important, that an occasional error resulting from its adoption will be readily overlooked by all concerned.

Rigidity of the abdominal muscles, more marked upon the right side, is an important symptom, and so is intestinal paresis, as indicated by inability to pass off the gas by the rectum. Pulse, temperature, vomiting, restlessness and prostration will also demand attention. These symptoms, however, are all subordinate in importance for purposes of establishing a probable diagnosis to those above mentioned, namely, pain and tenderness, which alone are worthy of the first and most careful consideration. A person may be very sick with appendicitis, and still have nothing the matter with him, if the pulse and temperament alone are depended upon as an indication of his condition.

Occasionally a case is met with in which the symptoms are so slight and so evanescent, that it is only after observation of repeated attacks that the character of the affection can be ascertained with reasonable certainty.

Since this paper was written, a young woman was admitted to the City Hospital with what was supposed to be appendicitis. She had the symptoms of a pretty severe general peritonitis of two days' duration. It was the third attack within two months. There was sudden, severe pain in the abdomen, followed by vomiting, retention of flatus, fever and restlessness; tenderness was more marked on the left side of the median line. She had severe exacerbations every twelve hours.

On opening the peritoneal cavity the appendix was small, moderately congested, and surrounded by recent adhesions. It was evidently not the focus of the present trouble, as the greater amount of congestion and discoloration was located in the region of the umbilicus. Here was found a firm band attached to a coil of the small intestines, and running down toward the pubes. This band was separated from the bowel, the appendix was removed, and the wound closed completely. She has made a satisfactory recovery. The only peculiarity of the symptoms in this case at the time of the operation, was that she did not seem to be quite as tender over the appendiceal region as you would expect her to be.

This case is mentioned merely to show, that occasionally on opening the peritoneal cavity for supposed lesion of the appendix, we do not find the exact conditions that the symptoms would lead us to expect. It does not militate against the treatment in any way, but on the contrary it illustrates the wisdom of exploratory operations under certain circumstances.

The diagnosis of probable appendicitis having been established, the treatment of the patient is the next question to engage the attention of the family physician. This is the time, in the majority of instances, when he needs and should have the aid and the support of the surgeon. I want to say here and now in the strongest possible manner, that every case of probable or possible appendicitis in which the symptoms are grave or protracted had better be seen by one who is familiar with the disease and who is accustomed to operating for its relief, for the following reasons: (1) To confirm or establish the diagnosis; (2) to divide and share the responsibility; (3) to get at the real condition of the patient at the earliest moment practicable, that his measure, so to speak, may be taken before he gets into a dangerous, and hence unsatisfactory, condition; (4) that the patient may have the benefit of early observation by the one who is to decide upon the necessity of an operation and the proper time for its performance; and, finally, for the satisfaction of the friends, as well as for the protection of the regular attendant in the future in case the result should be disastrous. It is seldom that a useless or premature consultation is called in this disease. Too often the surgeon, through no fault of the family physician, is summoned too late to be of any service, except to convey to the anxious friends a statement of the unfortunate condition of affairs, and to offer what consolation he may under the circumstances. Early, and if necessary, frequent consultations should be the rule in all but the very slightest cases of appendicitis.

The laity are now so familiar with this affection, and have such a horror of it, and justly so, that any error in judgment in this direction is not laid up against us. Hence I would urge all physicians who are not pretty familiar with this disease, and who do not feel reasonably certain of their patient's condition, to call a surgeon early. Put him on to the case in line with yourself; keep him posted as to the progress of the symptoms, and summon him whenever you are in doubt as to the true state of affairs. See your patient every six or twelve hours. Because he is comfortable at one visit, do not take it for granted that he will be at your next.

Look out for exacerbations. They mean a fresh extension of the infection, a further invasion of the peritoneum, and a greater menace to the welfare of the patient. You are dealing with one of the most treacherous of known diseases, insidious in its manifestations, uncertain in its career, and liable to sudden changes, which at any moment may put the patient in a condition of extreme peril. Given a case of appendicitis of any severity, your vigilance cannot be too active nor your precautions too complete to guard the patient against the various accidents and misfortunes liable to befall him.

I may here be permitted to digress for a few moments to speak more at length upon the proper treatment to be pursued in the earliest stages of this affection. You are called to a person suffering from pain and tenderness in the abdomen, which in your opinion indicates a lesion of the appendix. What shall be your first treatment? Will you give laxatives or an opiate? Many physicians would recommend the former, most surgeons the latter. If the pain is at all severe, I not only advise, but I urge you to give sufficient morphine under the skin to relieve the pain. That is the symptom that you are called upon to treat, and for the time being it overshadows everything else. Relieve the immediate distress, and then take time to consider as to what further treatment is necessary. The amount of opiate required to effect this object varies in different cases, and is often an index of the severity of the attack, and of the danger to be apprehended.

In fortunate cases the pain once subdued in this manner does not return. as was shown in the following instance: A young gentleman was recently seized with severe pain in the bowels, while in the recitation-room at college. He was obliged to leave tthe class, and started for home. The pain became so unbearable that he got off the electric car, crawled into the nearest physician's office, threw himself upon the floor, and rolled in agony. A half grain of morphine under the skin relieved him completely in a short time. He at once entered the City Hospital, and remained about a week, having no further trouble, save a slight tenderness or uneasiness in the appendiceal region upon deep pressure or in certain movements.

This gentleman was naturaly anxious about the future. He was told that in our opinion he was liable to have another attack at almost any time, and that it might be even more severe than the present one, and less readily amenable to treatment. It was for him to decide whether he would risk it or not. Our advice was that he should wait a few weeks or until every vestige of this attack had passed away. and then have the appendix removed. The operation was done inside of a month, and the appendix was found to be the seat of marked catarrhal inflammation. He made a quick and satisfactory recovery.

Now, no judicious physician could hesitate for an instant on purely theoretical grounds to give morphine, or some other quick opiate, in cases like this. To abolish the pain, remove suffering, and thereby save nerve force is plainly our first duty. As it requires a long time for sufficient opiate to be absorbed from the stomach of a person in great pain, it is far better to put it under the skin in the form of morphia. It is thus quickly and surely taken up, and we are not long left in doubt as to its effects.

So much for the initial treatment of appendicitis in the severe cases. What shall it be in those instances, of which there are many, where the pain is moderate in severity, and hence the necessity for immediate relief is not so urgent? Shall opium or laxatives be given? Experience teaches us beyond a doubt, that many cases recover under both methods of treatment; and yet, in my opinion, there is a very decided preference in favor of the former or sedative plan. Having been the victim to this disease at intervals for eighteen years, and having twice undergone laparotomy for its relief, the writer feels that he can express an opinion upon this point with some confidence. While relief may follow the administration of castor oil or other laxative in the lighter cases, yet the severe ones are more often made worse by such medication. Opium judiciously given seldom, if ever, does harm under these circumstances; laxatives oftentimes do great injury, and may turn the scale against recovery. There can be little doubt that the former is the safer and better plan. The writer claims that opiates will relieve every case that laxatives or cathartics will, and very many cases that the latter will not relieve. For these reasons he cannot urge you too strongly to depend upon opiates for the relief of pain in the first stages of appendicitis. It is claimed that opiates mask the symptoms in this affection, and may thus mislead the physician as to the patient's real condition. While this may be true to a limited extent, yet any one accustomed to seeing and studying this disease can form a reasonably correct opinion of the case by making due allowance for the effects of the drug. In rare instances a few hours' delay might be desirable, before a final decision is reached. At all events this objection is not often a valid one in every-day practice.

For the pain of appendicitis, then, opiates are our main reliance in our earliest stages, but we must not depend upon them for too long a period, or the golden opportunity for permanent relief may pass away never to return. Here comes in one of the most difficult questions of the whole subject for decision, to wit, When shall medical treatment be supplanted by surgical, or, in other words, when shall an operation be resorted to?

The indications may be briefly stated as follows: In very severe cases an operation is called for within twenty-four hours, or, at the outside, forty-eight hours from the commencement of the seizure. In cases of moderate severity, with no signs of amelioration of the symptoms on the third or fourth day, an operation is usually indicated. In relapsing cases, in from seven to fourteen days, according to the urgency of the symptoms. In recurring cases, after the first attack is entirely recovered from, say in from four to eight weeks, if the symptoms were severe, or after the second attack if the symptoms were only of moderate severity. It is to be borne in mind that these periods are only approximate, and not intended to be exact by any means, as each case is to be decided upon its own merits and manifestations. No affection calls for more careful consideration, a clearer understanding and calmer judgment than appendicitis.

It may be said here, that persons having a damaged appendix should be careful about traveling in foreign countries, as the physicians do not seem to be as familiar with this disease as are the American physicians. It is said to be more prevalent here than in other countries for reasons not yet satisfactorily explained.

In as brief a manner as possible, the particular conditions and circumstances in which a surgeon should be summoned in consultation will now receive consideration.

(1) In cases of the fulminating variety of appendicitis, in which the symptoms are always grave, and generally becoming worse from hour to hour to an alarming degree, it is scarcely possible to obtain surgical aid too early in the disease. An operation may, or may not, be necessary, according to the condition of the patient; but from the very nature of things, an experienced surgeon is the best able to decide that point. No one would think of operating upon a person while in a state of profound collapse. The proper treatment for one in that condition is hypodermic and rectal stimulation and external heat. If he cannot be rallied in that way, he certainly cannot be by an operation. Profound prostration, which differs from collapse in that the clammy sweats, cold extremities, flickering pulse and sighing respiration are lacking, may call for an immediate operation. By speedily relieving the system of the pent-up septic materials the vital powers are thus enabled to rally, and to regain their normal condition. Hence in these cases of sudden and severe seizure, you want all the aid you can get, and you want it at once. Don't wait till to-morrow.

It may be said here, that, contrary to what might be inferred from some writers of the present day, an operation, however early it may be performed, will not save every case of appendicitis. A small proportion of the victims of this disease will perish whatever may be the treatment, and however promptly and thoroughly it may have been adopted. Operations performed within twelve hours of the onset of appendicitis have failed to give the slightest relief. And yet this fact should not discourage early operations, when they are indicated, for in very many instances the convalescence dates from the time of the operation. It is simply a reminder that too much must not be expected from surgical measures.

(2) The greater proportion of cases of appendicitis met with in ordinary practice will probably come under this head. The symptoms are moderate in severity at first, but steadily grow worse every twelve hours

or so, with perhaps occasional remissions. These cases are often very deceptive. The patient does not seem to be very sick. With an occasional opiate, or perhaps without anything of the sort, he suffers little pain, and the attendants and friends are loath to believe that a serious, if not a dangerous, process is going on in the abdomen, until the vital powers are so exhausted that the patient's chances for recovery are very materially diminished.

In these cases I should strongly urge that the surgeon be called not later than the third day. Very likely the symptoms will be only of moderate severity at that time; but the consultant can form a much more correct and satisfactory idea of the character of the disease before its manifestations have reached the point of danger. He will also be better prepared to attack the case at the proper moment; in fact, he can choose his own time for interference. It is hardly possible to attach too much importance to this point. If the symptoms do not show marked amelioration by the third day, further counsel should be saught, not that an operation may be demanded at once, but that due preparations may be made, and that no time may be lost when time is important.

(3) A patient has a moderate attack of appendicitis. He is to all appearances improving, when without any definite reason, the symptoms are aggravated. He has a relapse, and has to make another start in his convalescence, only to experience the same chain of events sooner or later to prevent his recovery. The patient, and oftentimes the physician, is inclined to attribute these relapses to over-exertion, an error in diet, catching cold, etc. While these factors may occasionally act as exciting causes, yet the essential feature to be borne in mind is a damaged appendix.

Every exacerbation means an extension of the morbid process, and leaves the patient a little weaker and a little worse off, than he was before. The time may come when he fails to rally, and he then becomes an invalid from chronic appendicitis.

These cases are not quite free from danger, and sound judgment is required to decide the proper period for interference. Each case must be managed according to its condition and peculiarities, yet it may be said that, as a rule, two, or at most, three or four relapses, according to the severity, are sufficient to call for removal of the appendix. I presume that every surgeon feels that he has delayed action too long in occasional instances, but I have yet to meet one who has cause to regret a premature operation under these circumstances. It is better to err on the side of safety.

(4) No variety of appendicitis is more embarrassing to the attending physician, or to the consultant, than the following: A man is seized with a sharp attack of pain in the umbilical region, accompanied by tenderness.

It knocks him out, so to speak, doubles him up, and sends him to bed. The symptoms may be severe enough to produce a moderate collapse. An opiate under the skin gives him complete relief for several hours. Seen for the first time during this period, the patient seems to have little or nothing the matter with him. No pain nor tenderness, no disturbance of pulse or temperature. Everything is quiet under the sway of the "divine poppy." In many instances this lull in the symptoms is deceptive, as in from twelve to twenty-four hours they return with increased violence. This is usually the signal for calling the surgeon, and it goes without saying, that he should be prepared to operate at once if necessary.

(5) And, finally, we have the recurring cases of appendicitis to consider, which differ from the relapsing variety in the fact that patients are apparently well between the attacks. Recently a gentleman consulted the writer for the following symptoms: Five or six times during the past year, after a hearty meal he has been suddenly seized with a severe pain in the umbilical region, which gradually worked down into the vicinity of the appendix. The pain is accompanied by marked local tenderness and inability to stand erect. It lasts about twelve hours, but the tenderness persists for three or four days or even longer. He is weak and exhausted for several days—much more so than would be expected from an ordinary attack of colic or indigestion. This man is strong, robust, and is entirely well between these attacks. In the reader's opinion he is suffering from a mild form of recurring catarrhal appendicitis. Now comes the important question of treatment. Is an operation necessary, and if so, when shall it be performed? The temperament of the surgeon and of the patient is a factor in deciding these questions. If the attacks are increasing in frequency or in severity, there can be little doubt as to the necessity of an operation; and the sooner it is done the safer and better for the patient. On the contrary, if the intervals are increasing in length, and the symptoms become less severe and of shorter duration with each attack, it might be well to wait a while before resorting to a radical cure, as a certain number of these cases do eventually wear themselves out and entirely disappear, thus coming under the head of "appendicitis obliterans." In the opinion of the writer six attacks of appendicitis in one year, even if mild, are quite sufficient to justify an operation for the removal of the exciting cause.

Not a few physicians entertain the idea that there is no occasion for calling the surgeon until a tumor has formed, indicating that the inflammatory process is limited in extent and has ceased spreading. That this idea is erroneous and misleading, is abundantly proven by the fact that in very many cases no tumor is ever found, and yet the convalescence dates from the moment of operation. It is often impossible to detect a tumor, even when present, by reason of its depth in the pelvis, its shape and size, as well as the thickness and rigidity of the abdominal walls. The presence of a tumor does not of itself indicate an operation, nor does its absence preclude it. In fact, the indications for treatment are influenced very little by this symptom. A tumor shows the variety of the inflammatory processes going on in the peritoneal cavity, and also indicates the site of the incision. The plan of treatment is based upon other and more important factors in the condition of the patient. If he is growing steadily worse, radical means are called for, and the more rapid is the progress of the symptoms, the earlier are effective measures demanded.

In closing I can but repeat what has already been said, that from a surgical standpoint it seems wise and prudent that every case of appendicitis of any severity or duration should be seen in the early stages by one accustomed to operating for this affection, as well as to deciding the many difficult questions which are constantly arising throughout its course.

If the initiatory symptoms were severe, if they are steadily growing worse; if they relapse, or come to a stand-still; if the patient is sick, weak, irritable, impatient, restless, and especially if he cannot pass wind, or is inclined to nausea, vomiting, hiccough or delirium—then I urge you to be vigilant and prompt in calling for surgical aid, for reasons already mentioned. You will never be criticised for calling it too early or too often. The serious character of the affection, the sudden onset, the insidious course, the rapid and unexpected variations, the startling collapses and excruciating pain liable to occur in this affection, as in almost no other, will protect you from these charges. And should the result be unfortunate, you and the friends will have the lasting satisfaction of knowing that you have done your whole duty in the matter, and are in no way responsible for the disastrous termination.—Boston Medical and Surgical Journal.

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ABSTRACTS.

COMPARATIVE VALUE OF ASPIRATION AND THE SUPRA-PUBIC INCISION IN ACUTE RETENTION.

Dr. Legueu (Ann. d. mal. d. org. genurin) thinks the circumstances under which we are called upon to make a choice between these two methods for the relief of acute retention are exceptional, as only rarely is catheterism rendered impossible. He has had only nine cases in five years. These two resources have their advantage and their drawbacks.

As to aspiration, he has never seen a case followed by infiltration of urine. This may be avoided by using a trocar of small caliber, and on withdrawal first allowing air to enter the receiver and withdrawing the trocar by a quick movement. He thinks the objection that it is a blind method on account of possible danger to peritoneum of not much importance, there being only two or three cases at most in which it was established that the peritoneum was adherent to the symphysis, and in retention, of course, the peritoneum reaches its highest point. The objection that aspiration is purely a palliative measure and gives result for the moment only, he regards as one of its principal advantages. In diminishing the vesical distension the perivesical congestion abates and often some hours after, catheterism previously impossible, may be easily performed.

Supra-pubic incision is not altogether comparable. The future of the patient is not brilliant, and when the bladder is closed he has undergone in the meantime a traumatism, an operation which is not comparable either as to gravity or results with aspiration, therefore, except in bladder infection, when the value of the operation is not to be disputed, it has no immediate parallel with vesical puncture, and should be employed only when the nature of the obstacle or the gravity of the case permits the hope that there is more to be accomplished than emptying the bladder. Each method has its indications. There are three great causes in which there is a choice, hypertrophy of prostate, strictures, traumatisms.

I. In retention due to prostatic, hypertropy, aspiration seems always indicated when, the urethra being for the moment impassable, there is neither infection nor false passage. After evacuation, the congestion is lessened and it is often possible to practice catheterism, before impossible. Aspiration may be repeated: when we have warded off the immediate and most urgent accidents there will always be time to have recourse later to operation. The latter may be indicated under one of three conditions; when there is a false passage, when infection is present, or when, in spite of repeated punctures, the canal remains still impermeable to the catheter.

II. In stricture of the urethra, complicated by acute retention, each procedure has its indications. For stricture of the anterior urethra whether of gonorrheal or trumatic origin, external urethrotomy and resection when necessary is the operation of choice. Aspiration under these conditions is a means of delay only, in order to defer for some hours an operation the necessity of which is not to be disputed. The supra-pubic operation here is never indicated except secondarily, when retrograde catheterism seems necessary, to find the posterior end of the urethra.

In structures of the posterior urethra, which are always of traumatic origin, we may propose on principle the necessity of a supra-pubic incision to practice retrograde catheterization. In these cases external urethrotomy without a guide is particularly difficult. Legueu, in two such cases, did this operation before having recourse to the perineal cut.

III. In acute retention in consequence of a traumatism of the urethra, a perineal operation at the site of the rupture is the first to be thought of in all cases. If catheterization is impossible, we may consider the rupture as complete. We should not await the return of spontaneous urination and the ultimate cicatrization of the urethra, temporizing with repeated aspirations. On the other hand, supra-pubic incision is absolutely useless in the majority of cases; when the rupture occurs in the anterior urethra we can almost always find the posterior end. Legueu in one case where the urethra was stripped up by a catheter, the laceration being back of bulb, fearing a long search for the posterior end, did the supra-pubic operation at once and placed a catheter a demeure. The case recovered without accident, still he is not convinced of the necessity for the operation.

When the rupture occurs in the posterior urethra, which is the consequence of fracture of the pelvis, then the question of supra-pubic operation is debatable.—Journal of Cutaneous and Genito Urinary Diseases.

A CONTRIBUTION TO THE THERAPEUTICS OF IRON.

By DR. GELLHORN,

Assistant Physician to the Private Gynæcological Clinic of Dr. Mackenrodt, Berlin.

The skeptical assertions of Dr. Bunge, regarding the value of ferruginous medication, at the Congress for Internal Medicine of 1895, evoked an almost unanimous and vigorous opposition in the discussion which followed the reading of his paper. The doubts expressed by him in reference to an insufficient absorption of the inorganic preparations of iron could at that time only be controverted, in the main, by the results of practical experience derived from the administration of iron. However, Quincke even then pointed out that his investigations on the subject, which had not yet been concluded, had demonstrated the absorption of iron preparations given for medicinal purposes, and their utilization in the body. In 1896, at the Congress for Internal Medicine, Quincke reported the results of his experiments, which meanwhile had been completed, and which confirmed in every respect the above-mentioned statement. He had made it his aim to trace the course of iron along the intestinal canal, by means of micro-chemical reactions, and for this purpose fed white mice for a number of days with cheese, to which had been added various ferruginous preparations. The animals were killed during feeding, or after the lapse of a certain interval, and the viscera, especially the intestinal canal, hardened in alcohol, cut open and examined for the presence of iron with sulphide of ammonium as a reagent. It was thus found that iron is absorbed exclusively in the duodenum, and this applies both to the iron in the food and that administered medicinally. It was detected in the duodenal epithelium and in the stroma of the villi, and is visible even to the naked eye. Furthermore, iron is found deposited especially in the liver cells, in a form perceptible on microscopical examination, and in rare cases could be detected by microscopical means in the cortical tubules of the kidneys.

These investigations of Quincke have demonstrated incontestably that the favorable results which have been obtained, since olden times, from the administration of iron are actually attributable to its absorption, and not, as Bunge would have it, to accidental circumstances, to diet alone, or even suggestion. Control experiments in this direction with indifferent medicaments are readily carried out, and were repeatedly mentioned at the Congress of 1895. It should be added that these control experiments were followed by no change, or only by a transient improvement in the condition of the patient.

At the last Congress for Internal Medicine the subject of the therapeutics of iron was so thoroughly ventilated by the foremost clinicians, as well as by numerous physicians in late years, that a new contribution would appear superfluous. This subject, however, is of such immense importance to the general practitioner, that a cumulation of material is necessary in order to eliminate the least doubt as to the efficacy of a therapeutic measure which, originating at first on the basis of speculation, and later supported by the results of empirical observations, has finally been demonstrated to be of value by exact experimentation.

In the following I will only discuss the clinical aspects of this question. I was encouraged in undertaking this work by my honored teacher, Dr. Mackenrodt, who has assisted me in every possible way. In the management of chlorosis and anæmia, and the host of sequelæ of these diseases, the physician would be powerless if he had not in iron a specific, or at least a potent and indispensable adjunct to his other therapeutic resources. The patients, who belong for the most part to the working classes, give in the main the same group of symptoms: amenorrhœa, scanty or profuse, weakening, irregular, usually premature, menses; headache, anorexia and dyspepsia; neuralgias, and almost invariably marked lassitude, which interferes markedly with their ability to work. In these cases prompt and radical help must be afforded, in order to restore to the patients their full working capacity as soon as possible. It is well known that the therapeutic value of the various iron preparations differs greatly. This is shown a priori by the abundance of manufactured products of this kind. My experience relates chiefly to three preparations, pilulæ chinini cum ferro, formula magistralis of Berlin, liquor ferri albuminati, and the neutral Pepto-Mangan (Gude). My results with the first of these three remedies have been very indifferent, while with the liquor ferri albuminati of the pharmacopœia they were somewhat better. I have instituted accurate examinations, however, with only Gude's Pepto-Mangan, and the data given further on relate to this remedy alone. Owing to my limited experience with the many other preparations employed by various authors, I would not designate the Pepto-Mangan as a universal remedy, or as the only efficient preparation.

Still another remark: There can be no doubt that our medical intervention, no matter of what kind, is materially assisted by psychical impressions. This applies especially to our female patients, who are extremely susceptible to mental influences of this character. Hence, it may readily occur at the commencement of treatment that the previous disorders are less strongly felt, and it is therefore unfortunate that an objective criterion for the existing improvement is not at our disposal, as such we would regard regular examinations of the quantity of hæmoglobin in the blood. In the observations reported these were made with Gower's hæmoglobinometer. This instrument is very convenient, and is superior to Fleischl's apparatus for the use of the general practitioner, especially on account of its cheaper cost. The tests are very exact; any existing errors are the less to be considered, since they occur uniformly and in about the same degree during the entire course of the experiments.

That dietetic treatment alone may be successful in anæmic and chlorotic patients was laid down as a dictum by Immermann and Reinert at the Congress for Internal Medicine of 1895. It is natural to suppose that poor and ill-nourished persons would gain in strength under the influence of a proper and invigorating diet; nevertheless, after eight to fourteen days a cessation in the improvement occurs and the old disorders return. These authors, as well as Nothnagel and v. Ziemssen, consider an invigorating diet as only a valuable adjunct; both of the latter, moreover, regard rest in bed for several weeks as an important factor in the cure. Since several years Mackenrodt has also instituted a large series of observations of this kind, not yet published, in which, for purposes of control, he employed quantitative estimation of the hæmoglobin. It was found by him that under the influence of hygienic and dietetic regulations alone the quantity of hæmoglobin in the blood increased only at the commencement of treatment, and then only in a dilatory manner.

In the case of one of my patients I proceeded as follows: I prescribed Pepto-Mangan (Gude), one teaspoonful three times daily after meals, and regulated the diet in accordance with the directions furnished with the preparation. Sour and fatty foods, as well as raw fruits, are to be avoided under all circumstances. Fritsch (Diseases of Women, 1892, p. 469) advises, indeed, that the desire for acids manifested by chlorotics should be gratified. According to my experience, however, this craving for acids is to be regarded as a pathological condition of the alimentary tract, which is made worse by further supply of acids, but can be successfully overcome by an unstimulating diet. In cases where the social conditions in any way permitted, I allowed the patient to take a small glass of red wine three times daily, but never during a period of one hour before and after the administration of the medicament, in order to prevent the combination of the tannic acid contained in the wine with the iron. The use of potatoes was restricted as much as possible, at least during the first four weeks. Furthermore, I resorted to the dietetic regulations customary in these cases, but changed them to advantage when, as so often happens, obstinate constipation was present, following in this respect the suggestions of Hebra, which have recently been again advocated by Ruge (Transactions of the Obstetrical and Gynæcological Society of Berlin, 13, III, 1896), and obtained generally excellent results. In contrast to several authors who made it a practice to remove any existing dyspepsia before resorting to the use of iron, I have followed the method of v. Ziemssen and Baumler, of at once administering iron—unless the presence of a severe gastric affection, especially ulcer of the stomach, could be positively determined—and observed as early as the end of one or two weeks an increase of appetite and subsidence of the gastric disorder.

I would lay especial stress upon systematic exercise in the open air. I ordered the patients, who, with but two exceptions, were treated out of bed, to take a stroll at midday, at first of five to ten minutes' duration. At the end of three to four days they were allowed to remain outdoors for five to ten minutes longer.

After each walk they were advised to take off their corsets, put on their slippers, and rest for an hour on the sofa. Under this treatment, the lassitude invariably vanished after a time.

In the manner thus described I have treated in all about sixty patients. In twenty-four cases I instituted quantitative estimations of hæmoglobin at regular intervals of three, five, or eight days. Under normal conditions the quantity of hæmoglobin in woman amounts to 12.59 per cent. when estimated in comparison with the other constituents of the blood. Among my cases the lowest amount met with was, in a single instance, 30 per cent. of the normal, that is to say, of the above 12.59 per cent. Next to this was the following case with 32 per cent. of the normal:

Miss W. G., twenty-two years old, seamstress, related that she had been under treatment for four years for chlorosis. Since the age of nineteen, her menses had been scanty, occurring before the usual time, and of three to eight days' duration. On September 26, 1895 a remotio secundinarum occurred after an abortion induced in the fourth month. At present she complains of darting pains in the upper portions of the lungs, headaches, and rapid loss of strength.

January 9, 1896, anæmic appearance; physical examination, especially of lungs, negative. Quantity of hæmoglobin 32 per cent. Ordered Pepto-Mangan, Gude, diet, etc.

January 13, 1896, considerable improvement of the general condition. Hæmoglobin, 45 per cent.

January 17, since previous day, diarrhœa, due to gross errors in diet, troublesome eructations. Ordered tinct. opii, 15 drops three times daily. Hæmoglobin, 47 per cent.

January 21, improved after use of tinct. opii, no more gastric pains or eructations. Headaches have completely disappeared, lassitude less marked. Hæmoglobin, 55 per cent.

January 31, condition unchanged, ceased menstruating on previous day, the flow having lasted five days.

February 8-28, patient feels well and no longer complains of pains in the lungs. Appetite and bowels regular. Hæmoglobin, constantly 55 per cent.

March 5, no change. Hæmoglobin, 62 per cent.

March 11, hæmoglobin, 68 per cent. March 27, 77½ per cent.

Unfortunately, as in most of these cases, the patient's visits ceased as soon as she felt entirely capable of going to work. As a matter of fact, the increase of hæmoglobin in this case was tardy, as in four other cases in which the quantity at the beginning was 34, 35, 37, and 38 per cent. of the normal. In eighteen other instances in which the initial amount was higher, viz., 42-75 per cent. of the normal, progress was more rapid as a rule.

This is most strikingly illustrated in the following case:

Miss C. B., aged fifteen years, complains of violent headaches, visual disorders, loss of appetite, a feeling of pressure over the stomach, constipation, and general lassitude.

June 2, 1896, status præsens; mucous membranes pale; physical examination, negative; heart, normal; quantity of hæmoglobin, 45 per cent. Prescribed as in above case.

June 9, headache has disappeared; condition otherwise unchanged. Hæmoglobin, 45 per cent.

June 16, improvement. Hæmoglobin, 51 per cent.

June 23, decided improvement. Hæmoglobin, 55 per cent.

July 8, patient free from complaints; cheeks ruddy, lips and conjunctiva red. Hæmoglobin, 78 per cent.

July 23, and September 24, continued good health.

I also derived exceedingly favorable results from the use of Pepto-Mangan, Gude, in patients who came to us for operations after having been exhausted by protracted hæmorrhages. Of course convalescence in such cases is delayed; the system recuperates but slowly from the double injury inflicted by the losses of blood and the operative intervention. Digestive disturbances are especially apt to be troublesome. In these cases ferruginous medication often produces remarkable improvement.

I cannot close this paper without calling attention to the beneficial influence exerted by Pepto-Mangan, Gude, in anæmic neuralgias, and as an illustration of its effects in this class of cases add in brief the following history of a case:

Mrs. K., aged thirty-five years, very pale and ill-nourished, suffers from intercostal neuralgia on the left side.

January 30, 1896, quantity of hæmoglobin, 68 per cent. of the normal.

February 5. In the meantime has suffered on two days with violent headaches. Intercostal neuralgia persists. Appetite good; no gastric disturbances. Hæmoglobin, 69 per cent.

February 12, no longer troubled with headaches with exception of one attack of neuralgia in the area supplied by the left supra-orbital nerve. The paroxysms of pain on the left side of the chest have become less frequent. The lassitude has subsided. The mucous membranes are still anæmic. On the whole, the patient feels better and more vigorous than before the commencement of treatment. Hæmoglobin, 75 per cent.

February 18, considerable improvement of neuralgias; no headaches, nor digestive disturbances. General health improved. Menses appear earlier than previously, this being the second day of the flow. Hæmoglobin, 73 per cent.

February 26. During the preceding days transient deterioration of her conditions owing to mental excitement. Menstrual period has been normal. Hæmoglobin, not estimated.

March 2, patient no longer complains. Intercostal neuralgias have ceased to occur except on rare occasions. Hæmoglobin, 76 per cent. March 13, health good in general. Iron discontinued on account of gastric disturbances which are said to result from excitement. Ordered strict diet and iron to be resumed.

March 19, complete restoration of health. Hæmoglobin, 82 per cent.

That the final estimates did not yield the normal quantity is not surprising, since it is frequently somewhat reduced even in healthy persons. At any rate, the objective and subjective state of the patients in the above cases, as well as in the others not reported in detail, afforded the impression that a radical cure with complete restoration of the ability to work has been effected.

It must be conceded that in matters of therapeutics it is always difficult to appreciate correctly the relation of cause and effect, and to eliminate the factor of accidents in estimating the efficiency of any plan of treatment. And in order to arrive at a positive and unbiased decision, it is necessary to resort to a series of observations and control experiments of so great an extent that the single observer, even though he have at his disposal a vast amount of material, is only capable of furnishing a small contribution in the discussion of these questions. Furthermore, a certain amount of latitude must always be allowed to individual judgment.

Yet, while fully conscious of these limitations, I think I am justified in asserting that in my therapeutic trials with Pepto-Mangan I obtained all that can be rationally demanded. And I further consider myself warranted in stating that in view of the unquestionable necessity of ferruginous medication in certain troublesome constitutional affections this preparation acts as a most efficient and useful auxiliary to our therapeutic efforts.—Therapeutische Monatshefte, May, 1897.

CONCERNING EUCAINE HYDROCHLORATE "B" IN PRACTICAL OPHTHALMOLOGY.

By Dr. P. SILEX,

Privatdocent and First Assistant to the Clinic.

(Translated from the Deutsche Medicinische Wochenschrift, Feb. 4, 1897.)

Several months ago a new anæsthetic, Eucaine, was sent to the Ophthalmological Clinic of Professor Schweigger for trial. I made the experiments with it myself, and my conclusions were that it was not equal to cocaine. It did indeed cause anæsthesia of the cornea, but its instillation was followed by such violent burning, and by so great an injection of the conjunctiva bulbi, that I ceased using it after about 15 patients had been experimented with. Best,* who used it in the Giessen Clinic, also obtained no satisfactory results. Vinci's† work was purely scientific, and referred only in a few words to its practical application. From an opthalmological standpoint, therefore, the anæsthetic had but a minimal value; and thus chemistry was incited to discover a compound that would obtain more general recognition.

Such a one is Eucaine "B," of which considerable quantities were placed at my disposal.

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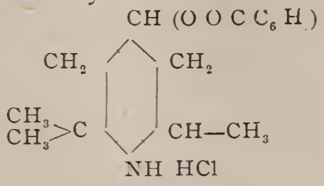
* Best, "Eucaine in Ophthalmology." Deutsche Medicinische Wochenschrift, 1896, p. 573.

† Vinci, "Concerning Eucaine, a new local anæsthetic." Virchow's Archiv. für Pathologische Anatomie und Physiologie, 1896., vol. cxlv.

Eucaine "B" is the hydrochlorate of benzoylvinyldiacetonalkamine, whose constitution is expressed by the formula:



It is closely related not only to the older Eucaine "A," but also to cocaine and especially to tropacocaine; but it differs from the two last named bodies in that it is much less toxic. The hydrochlorate also differs from the similar cocaine salt in that it can be boiled for a long time without undergoing decomposition. Its solution can therefore be sterilized by boiling.

The solubility of the hydrochlorate of Eucaine "B" in water at about 70° F. is about 5 %. Dr. Vinci says 3 %; but this is not the case. The solution is of a neutral or very faintly alkaline reaction. It differs from Eucaine "A" in that its irritant effects are minimal, and are only seen in isolated cases. This circumstance, together with its very slight toxicity and its powerful anæsthetic action, render Eucaine "B" very valuable for a variety of purposes; amongst others, for the Schleich infiltration method and for subcutaneous injection in general.

Encouraged by its general good qualities, we experimented with the Eucaine "B" in regard to its local action, using it in 2 % watery solution on 1, normal eyes; 2, inflamed eyes; and, 3, operations upon the eyes.

1. I instilled cocaine (2 %) into one, and Eucaine "B" (2 %) into the other eye. Many patients experienced no burning at all from either instillation; in others the cocaine, and in others again the Eucaine "B" smarted more. To me personally cocaine was the more disagreeable. On the application of a second and a third drop after two and four minutes' interval, the results were again contradictory. Many patients affirmed that there was no pain to speak of at all.

The anæsthesia of the cornea was about the same in the same space of time in most of the cases. Eight times out of 31 observations I noted that insensibility came on more rapidly and was more marked in the eucainized eye. It appeared in from one to three minutes, and disappeared in 15 to 20 minutes. On the average two drops produced a marked anæsthesia in two minutes.

In all the cocainized eyes an even whitening of the sclera occurred, caused by the vascular constriction and the secondary anæmia; whilst the eucainized eyes in half the cases showed no changes of vascularization at all, and in the remainder merely a delicate pericorneal rose-colored injection and a dilation of the vessels of the connective tissue of the eyeball.

This dilation was always very moderate, and the patients never suffered any inconvenience at all from it. In both classes, however, anæsthesia was present. We think the phenomenon to be due to a contact paralysis of the superficial nerve endings.

The vascular dilatation in some of the cases appeared to be due to a paralysis of the sympathetic nervous system; for an irritation, such as occurs after instillation of cocaine, always causes narrowing of the lumen of the vessels; and the anæsthesia is probably due to the deficient nutritive supply that is thus occasioned. Whether the vascular dilatation is due to irritation of the vaso-dilators, or paralysis of the vaso-constrictors —both of which factors are capable of causing it—is a thing concerning which we have as yet no exact information.

From theoretical considerations I believe that cocaine as an irritant causes spasm, and Eucaine a paralysis, of the constrictors. The action of the latter drug is an entirely local one. Müller's muscle and the pupil are not acted on, as with cocaine. The size of the pupil is unchanged, and the sphincter reacts promptly to light and convergence. Three cases that were kept in a condition of complete anæsthesia by means of repeated instillations, for 20 minutes, showed not the slightest dilatation of the pupil. Vinci saw an enlargement of the aperture to the extent of $\frac{1}{2}$ to 1 mm. (1-50 to 1-25 inch) only after large doses. Accommodation was never affected.

In regard to any change in the intra-ocular pressure I can only say that I could find no diminution of it in the 30 normal eyes that were experimented with. Others have found this to occur with Eucaine "A;" perhaps they had a more delicate sense of touch than I possess, and perhaps it might have been apparent if a 31st eye had been tried. The disturbing influence of hypotonia in operative procedures after lengthy cocaine instillations is well known to every operator. If this is entirely absent, or but little marked with Eucaine "B," it would be a very great point in favor of the drug.

I have not observed any corneal cloudinesses that were visible to the naked eye. After the instillation of one or two drops of each solution into a pair of eyes, if they were held open for a few minutes, examination with the lens showed almost always roughness of the surface and the well-known system of irregular, wavy, broad lines in the cornea of the cocainized eye; whilst in the eucainized eye these appearances were entirely absent. An uncertain, delicate, veil-like cloudiness of the superficial layers could indeed be seen, but not so frequently as it appears with cocaine. If after the instillation the eyes were closed at once, or the lids covered with moist cotton, both sides usually remained intact; only occasionally could the above-mentioned lines be perceived in the cocainized eye.

Opinions still differ as to the reason for the appearance of the cloudiness after the use of cocaine; but practically it may be almost always avoided by keeping the eye moist. The employment of Eucaine "B" may enable us to reduce the number of cases of cloudiness of the cornea, since it causes no lymphanæmia of that tissue.

2. A large number of inflamed eyes were experimented with, including cases of foreign bodies in the cornea, losses of epithelium, corneal ulcerations, irritated interstitial keratitis, episcleritis, and iritis.

It seemed both to me and to the other physicians that worked with me, that, during the removal of foreign bodies from the eye, the patients had less sensibility with Eucaine than with cocaine. Conjunctival injection of cooking salt in episcleritis caused no trouble at all after a double instillation. Four corneal ulcers were cauterized. The injection of the bulb that sometimes occurred neither caused the patients any inconvenience, nor interfered with the operator in his manipulations. In short, the therapeutic behavior of the drug was exactly like that of cocaine.

In a corneal wound the size of a finger nail, with a large and deep loss of the epithelium, six drops of cocaine gave relief in 12 minutes. An hour later violent pain set in again, which was relieved by five drops of Eucaine "B," in 10 minutes, though the eye was now fiery red.

I must mention, however, that, in those affections that were accompanied by marked hyperæmia of the anterior segment of the eyeball, such as iritis, the patients preferred cocaine to Eucaine. The cocaine anæmia gave them relief. Cocaine and atropine was more pleasant also in these cases than Eucaine and atropine.

3. The remedy was tried during the extirpation of chalazion, section of the tear duct, one tenotomy, three iridectomies, three secondary and twelve senile cataracts. No difference could be appreciated by either the operator or the patient between the two drugs.

In the cataract operations six drops were administered during the ten minutes before they were begun. Some of the eyeballs became injected a little, others not at all. This is a matter of indifference, however, during a corneal section, and also, I think, during the formation of a conjunctival flap; a few drops of blood will in no way interfere with an operator.

Hypotony and clouding of the cornea did not occur; the eyes were kept covered. The number of cases is, however, as yet a small one.

The behavior of the patients when the iris was seized was very various. Just as occurs with cocaine, some complained of pain, and others did not.

To give a general opinion as to the value of the remedy, I can say, that, so far as the specimens used in the University Ophthalmological

Clinic are concerned, it fully satisfied all the demands that could properly be made of it. Patients affected with iritis, who have already had cocaine, will probably miss that drug. The unchanged pupil is an advantage in cataract operations where the iris can be better avoided than when it is dilated; for it often becomes difficult to pass the thick mass and make the incision into the limbus.

We can recommend the employment of Eucaine "B," and we think that it deserves a place in our armamentarium. It is always an advantage if we can replace the natural product of distant lands by a scientific chemical compound.

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PROCEEDINGS OF SOCIETIES.

AMERICAN MEDICAL ASSOCIATION.

Abstract of the Proceedings of the General Sessions of the Forty-eighth Annual Meeting, Philadelphia, Pa., June 1-4, 1897.

[Continued from July issue.]

FOURTH DAY. — FRIDAY.

The Association met at 10 A. M., and was called to order by President Senn.

Dr. John B. Hamilton, of Chicago, delivered the address in State Medicine. He selected for his subject,

THE PREVENTION OF TUBERCULOSIS.

He stood, he said, by appointment in the place of the late Dr. Jerome Cochran, of Montgomery, Ala., whose pre-eminence in the science of preventive medicine easily singled him out from among his fellows as peculiarly fitted to deliver the annual address in State Medicine. His vast erudition, extended experience, and long public service made him facile princeps in all matters relating to sanitary work. Dr. Hamilton then paid a glowing tribute to the life and work of Dr. Cochran.

Coming to the subject of his address, he said that pulmonary tuberculosis alone took 345,963 lives in the five years, 1850, 1860, 1870, 1880, and 1890. These were the years in which the United States census was compiled.

In the 28 largest cities of the German empire, the number of deaths from consumption per 100,000 of the aggregate population during the year 1890 was 299, ranging from 453 in Nuremberg, 367 in Dortmund, 357 in Frankfort-on-the-Main, 388 in Crefeld, and 353 in Elberfeld to 236 in Königsberg, 249 in Strasburg, 239 in Halle, and 247 in Chemnitz.

The average annual number of deaths from consumption per 100,000 of inhabitants for the 10 years, 1881 to 1890, was, for Paris, 463; Nuremberg, 457; Dortmund, 417; Barmen, 381; Aachen, 377; Frankfort-on-the-Main, 373; Crefeld, 373; Bremen, 392, and London, 206. The death rates from consumption in most of the large cities diminished during the ten years 1881 to 1890. The difference in the mortality from consumption in the large cities of the United States depend largely upon differences in the proportion of different races in the several cities, and, also, to some extent, upon the age, distribution and density of population, on the depth of the level and subsoil water, and on the dryness and uniformity of temperature of the climate. As a rule, the death rate from this cause in large cities is greater than it is in the small towns and rural districts.

General Measures of Relief.—The humanitarian turns to science for relief from this "great white plague," so destructive, so elusive, and so calamitous; and it is now the province of State Medicine alone to point out the methods by which we may put this plague "upon a basis of gradual extinction."

The prevention of tuberculosis cannot be accomplished in a day, but its ravages may be made less by such prompt measures on the part of our boards of health as may easily be taken, provided the laity are taken into confidence. The public must be fully educated to an understanding of the causes and method of propagation.

Heredity.—The heredity of the disease has been established by Baraud and Rénon, who reported to the Société de Biologie of Paris, in June, 1895, that they had investigated the question of the direct transmissibility of the bacillus through the placenta to the human fetus. In five cases they took a quantity of blood from the umbilical veins, and injected it under the skin of the abdomen of guinea pigs, immediately after the birth of the child. In three cases the guinea pigs did not become tuberculous, while in two they did. In the first case the mother was in the first stage of phthisis, the sputa containing bacilli. There were no lesions of the placenta present, apparently. The guinea pig which was injected developed a chancrous ulcer at the point of inoculation, and died with tuberculosis of the liver and spleen at the end of two months. The bacilli were found in the spleen, pulp and in the caseous masses of the chancrous ulcer. An autopsy performed on the still-born infant gave negative results. Three guinea pigs were injected with the juice of the pulp of the lung, liver and peritoneal serum, and two of them died. In the second case the mother had pulmonary cavities, but no examination of the sputa was made. The child died of broncho-pneumonia on the fortieth day. The placenta presented a normal appearance. At the moment of birth blood from the umbilical vein was injected into two guinea pigs; one pig died of generalized tuberculosis, the other lived.

Solly, of Colorado, holds that a connection between phthisis in an individual and phthisis in the family may be exercised in one of three ways, namely, by inheritance of bacilli, by inheritance of susceptibility, or by contagion.

Lehman has reported a case of congenital tuberculosis, where a mother died three days after the birth of the child, of tuberculous meningitis. The child lived but twenty-four hours. In its spleen, lungs, and liver were found nodules resembling tubercles and containing tubercle bacilli in large numbers.

Hahn has collected and reviewed some cases of tuberculosis where they were undoubtedly congenital. The semen may also become infected and thus transmit the disease to the fetus. Gärtner caused tuberculosis in young mice by injecting the mother with tubercle, either into the peritoneal cavity or into the blood stream.

Baumgarten detected the tubercle bacillus in the ovum of a female rabbit which he had artificially fecundated with tuberculous semen.

Infection by Milk.—There is no longer any question of the possibility of infecting the lower animals by the injection of tuberculous milk, but direct evidence of transmission of tuberculosis by milk in man by ingestion is not yet positive, except in cases of udder tuberculosis.

In cows with tubercular disease of the udder, the Royal Commission found that the milk was in every case infective, and the experiments made by inoculation in every case gave positive results. As regards feeding, 27 animals were fed with varying quantities of milk from cows with tubercular udders. Of these 27 animals, 19 developed tuberculosis. The brunt of the disease in these cases fell on the abdominal organs, showing the alimentary origin, but in some cases also there was a more generalized infection. The glands about the mouth and angle of the jaw were also found sometimes tubercular.

Dr. Martin, in speaking of these experiments, says: "The milk of cows with tuberculosis of the udder possesses a virulence which can only be described as extraordinary. In those cows where the tubercle bacilli were found in the milk, the feeding experiments were uniformly positive as well as the inoculation experiments. It is noticeable, too, that a small dose of the milk diluted four times gave tuberculosis to all the animals fed, and that a dose of even .05 to .1 cubic centimeter diluted with non-infective milk was sufficient to produce tuberculosis."[1]

It is well known that many cattle are affected with tuberculosis in this country, that the milk goes into the general supply, and that tubercle bacilli may be found in the milk in cases of udder tuberculosis, and that rabbits inoculated with infected milk or fed upon infected milk, become tuberculous. Dilution with non-infected milk attenuates the bacilli, and the dishonest practice of watering is, therefore, not without its compensation.

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[1] T. P. C. Kirkpatrick: The Spread of Tuberculosis by the Milk Supply, Dublin Journal Medical Science, May, 1897.

Dr. Gehrman, of the Chicago Health Department, after testing the milk from tuberculous cows, writes: "In only one specimen of milk was it possible to demonstrate the presence of the tubercle bacillus microscopically, but in thirty-eight injections of the milk in animals we had the transmission of tuberculosis demonstrated six times."

Infection by Tuberculous Meat.—Tubercle bacilli have been found in the flesh of horses, cattle, and swine. Birds and fowls also become infected, and when dogs and cats become infected it is said that they become so by reason of eating flesh of tuberculous animals, but this lacks corroboration by extended experiment.

The review of the recent reports on this branch of the subject corroborates the former view that careful cooking of meat and boiling of milk before eating or drinking these articles will prevent them from doing positive harm, but few would relish food if it were known to be tuberculous, even if cooked by a Lucullus. An act passed this year by the Maine Legislature provides for the killing of all diseased animals, the injection of kerosene oil into their cartoid arteries, and the burial of the carcasses within twenty-four hours or their reduction to fertilizing material within forty-eight hours.

To sum up, it appears that the greatest danger is from the dried sputum, and that other causes, while they exist, are of much less importance. The local infections that are found in the bones and internal viscera are produced by invasion through the circulation, while the infection of lymphatic glands doubtless takes place by direct absorption through the lymph channels. Every surgeon has noticed without doubt that tuberculous glands become successively affected; it is the exception to find a whole chain of glands simultaneously affected. When the tubercular lesion is found in the skin the point of invasion is an abrasion, or a center beginning in the hair follicle.

The means of prevention of the spread of tuberculosis are then sufficiently apparent after we have taken our observations on the methods of infection. The factors in successful prophylaxis are: (1) The guarding against infectious food or drink being issued to the people anywhere. (2) The prompt destruction of cattle proved to be tuberculous. (3) Proper notification to the sanitary officer of the occurrence of cases of pulmonary consumption.

Marriage of the Tuberculous Should be Opposed.—The law prevents marriage within certain degrees of consanguinity; no account is taken of the physical state of the contracting parties, although in many States certain physical causes are sufficient reason for divorce. If chronic and incurable disease be sufficient ground for separation, how much more reason is there for the prohibition of the marriage rite. The insane and incurably diseased should not be allowed to propagate their species, unless we confine our sympathy to the individual instead of the whole people. The heredity of the disease has at last been fairly proved by scientific investigation. Public opinion should now do the rest.

Richardson's "City of Hygeia," that beautiful fabric of a vision, as one of our countrymen has said, "was blown away by the winds, but they blew it all over the earth." So the wide dissemination of correct knowledge concerning the cause and propagation of tuberculosis cannot but tend to still further restrict its ravages, and thus add to the sum of human happiness and the prolongation of human life.

The following officers were elected for the ensuing year:

President, Dr. George M. Sternberg, Washington, D. C. First Vice-President, Dr. Joseph M. Mathews, Louisville, Ky. Second Vice-President, Dr. J. L. Thompson, Indianapolis, Ind. Third Vice-President, Dr. J. W. Wiggin, New York, N. Y. Fourth Vice-President, Dr. T. J. Happel, Trenton, Tenn. Treasurer, Dr. Henry P. Newman, Chicago, Ill. Assistant Secretary, Dr. W. A. Jayne, Denver, Col. Librarian, Dr. George W. Webster, Chicago, Ill. Chairman Committee of Arrangements, Dr. J. W. Graham, Denver, Col.

Board of Trustees: Dr. J. T. Priestley, Des Moines, Ia.; Dr. Joseph Eastman, Indianapolis, Ind.; Dr. Truman W. Miller, Chicago, Ill.

Judicial Council: Dr. D. W. Crouse, Waterloo, Ia.; Dr. T. D. Crothers, Hartford, Conn.; Dr. Wm. T. Bishop, Harrisburg, Pa.; Dr. R. C. Moore, Omaha, Neb.; Dr. G. B. Gillespie, Covington, Tenn.; Dr. C. H. Hughes, St. Louis, Mo.; Dr. Ida J. Hieberger, District of Columbia.

The annual addresses were allotted as follows: On General Medicine, Dr. J. H. Musser, Philadelphia, Pa.; on General Surgery, Dr. J. B. Murphy, Chicago, Ill.; on State Medicine, Dr. S. C. Busey, Washington, D. C.

Adjourned to meet in Denver, Col., in 1898.—Boston Medical and Surgical Journal.

RICHMOND ACADEMY OF MEDICINE AND SURGERY.

Regular Meeting held June 22, 1897.

Dr. J. N. Upshur, President, in the chair; Dr. Mark W. Peyser, Secretary and Reporter.

Dr. Mark W. Peyser read a paper on "Digestive Agents." [See page 67.]

DISCUSSION.

Dr. H. H. Levy thought Dr. Peyser correct concerning the too free use of pepsin and the pancreatic enzymes, and that stimulation, rather than help, was better for the proper activity of the digestive organs. The conditions for secretion should be made more favorable by administration of acids and alkalies. In conclusion, he said the test meals afforded a scientific diagnosis—whether the fault lay in hyperchloridia, hypochloridia, etc.

Dr. J. S. Wellford thought that, as trypsin and the other pancreatic enzymes were nitrogenous in character and, therefore, likely to be acted upon in the stomach, it was useless to give them, unless they could be so prepared as to be made invulnerable until the duodenum was reached.

Concerning pepsin, it should never be administered in combination with bismuth, as the latter exerted a katalytic action upon pepsin, which should always be given in an acid solution.

Dr. Levy wished to ask why pepsin, which was also nitrogenous, was not acted upon by the gastric juice?

Dr. Wellford replied that he could conceive of no other reason than that pepsin was a natural ingredient of the juice, and therefore not likely to be acted upon.

Dr. Peyser, in closing the discussion, said one of the very points that he wished brought out in his paper was involved in Dr. Wellford's remarks. The stomach, not being acid in the beginning of digestion, the pancreatic enzymes could be given in solution at the beginning of a meal with the almost perfect certainty that they would reach the duodenum before they could be acted upon in the stomach.

As for the balance of the paper, he was glad to know he was in such accord with the Academy.

Regular Meeting held May 11, 1897.

Dr. J. N. Upshur, President, in the chair; Dr. Mark W. Peyser, Secretary and Reporter.

Dr. W. S. Beazley read a paper on "Some of the Evils that Threaten the Medical Profession." [See page 69.]

REPORT OF CASES.

Dr. Geo. Ross reported the following cases:

Malignant Disease in Child's Mouth.—Boy, age 8 years. Father, mother and uncle died of consumption. Aunt has it. Grandmother died of apoplexy. An abscess in the lower jaw, due to a decayed molar, was opened several times, and, now, in the cavity, there has appeared a foreign growth, about the size of a guinea-egg, and apparently of a malignant character. He reported the case because of the rarity of malignant growths in childhood.

Myelitis and Paraplegia. Partial Recovery. Then Right and, Later, Left Hemiplegia. Paralysis of Diaphragm and Death.—Male, white, aged about 58 years; wore low quartered shoes the whole of one rainy day, and in the afternoon, without changing them, put on rubber shoes. The next morning he complained of chilliness and malaise. Walking became more and more difficult until the fifth day, when it was an impossibility to walk. A diagnosis of acute myelitis was made with paraplegia—paralysis of the bladder and rectum, etc. He removed South, remaining an ataxic, then went North, and consulted various authorities, Dr. Seguin among others.

About twelve days ago, while in the city again, he became absolutely paralyzed on the right side, and was taken to the Virginia Hospital. Six days ago there was a second paralytic attack involving the left side and the diaphragm. In twenty-four hours articulation became impossible, and he died.

The patient was a student and literary man, with no spyhilitic or gouty history, and of regular habits.

The President said Dr. Ross's case was very interesting, especially because of the absence of gouty or syphilitic history. He thought there must have been some condition, back of the myelitis, tending to produce tissue change. The patient might have inherited gout, and along with it slow changes in the vascular system, as deficiency of the elasticity of the vessels, or a change due to the cold going to the brain and slowly acting upon the nervous tissue.

He reported the following case:

Suppurative Salpingitis. Recovery Under Medical Treatment.—Woman, age 23 years; unmarried; seen January 26, at which time she had been confined to bed for five months. She complained of pain at all times; and between 12 M. and 3 P. M. of violent attacks in the hypogastrium.

There was also dysmenorrhœa. Digital examination revealed the fact that she was not a virgin. The uterus was firmly fixed in the pelvis, and its body could not be outlined. In the cul-de-sac, a mass was found as large as an orange, and boggy to the touch. A dirty discharge came from the cervix. There was occasional difficulty with micturition. After tonic treatment and the use of douches at home, she was taken to the Old Dominion Hospital for operation, and Dr. J. W. Long was called in consultation. They agreed that the case was suppurative salpingitis, due in all probability to gonorrhœa. Twice daily douches of normal saline solution, as hot as could be borne, were given, followed by the introduction of a tampon anointed with 25 per cent. ichthyol unguent.

At the end of six weeks, the effusion had entirely disappeared, and the only evidence of pelvic trouble was the dirty discharge.

Menstruation at this time was still irregular, but not attended by pain. Ten days ago the uterus was dilated and curetted (there being hardly any suppuration), and packed with iodoform gauze. In three days she returned, and very little discharge was seen. He now uses a syringe (capacity, one drachm) to inject pure peroxide of hydrogen, and then swabs out the uterus with ichthyol. Absolute recovery is evidenced, and that without operation. Tonic treatment consisted in the administration, three times daily, of a pill of manganese, iron, and nux vomica.

Dr. Jacob Michaux reported in detail a case of Abscess of the Kidney, originating from a neglected case of inflammation of the bladder. Urine secreted, although about one-half of kidney was taken up in an abscess cavity. [Dr. Michaux has the case still under treatment, and as the other kidney is becoming involved, he will reserve a full report of the case until the next issue of this journal.]

:o:

THERAPEUTICS.

SOME RECENT SUGGESTIONS IN THE TREATMENT OF EPILEPSY.

Since the introduction of potassium bromide in the treatment of epilepsy, by Sir Charles Locock, in 1857, no agent has been found so generally useful. It is admitted, however, that the bromide treatment leaves much to be desired, and therefore new remedies are brought forward continually, with the hope that they may prove more efficacious in controlling the paroxysms, and at the same time less active in producing untoward symptoms. It has been clearly shown that certain bromide preparations have a less deleterious effect on general nutrition than others, though the same salts may be, to some extent, less powerful; this is particularly true of strontium bromide, which is far less apt to induce acne, mental depression, and gastric disturbance than the corresponding salts of potassium. According to Wood, both ammonium and strontium are stimulants to the circulation, while potassium is a powerful depressant. The strontium salt differs from the ammonium salt in being directly less active as a bromide, but in having a more happy effect on the alimentary canal. As the cost of the strontium salt has been lately considerably reduced, its employment in epilepsy will doubtless become more general. Within the last few years three drugs have been introduced in the treatment of epilepsy, which, though of litttle value when administered independently, are very useful adjuvants, and when combined with a bromide often lessen materially the amount of the latter required to control the seizures. These three remedies are antipyrin, the fluid extract of *solanum Carolinense*, and opium. Antipyrin, in doses of ten or fifteen grains a day, can be given with ammonium or strontium bromide for several months without ill effects. In some instances, however, its prolonged use excites circulatory disturbances, such as coldness of the extremities, cyanosis, and copious perspiration. *Solanum Carolinense*, or horse-nettle, was first recommended by Napier. Potts (Therapeutic Gazette, December 10, 1895) draws the following conclusions from the use of this drug in seventeen cases of epilepsy: That the drug has a decided influence for good upon the epileptic paroxysms; that this influence is probably not so great or so sure as that obtained by the use of antipyrin and the bromide salts, or even of the mixed bromides; that in those cases in which it is of service it relieves the paroxysms without causing unpleasant symptoms, such as are sometimes caused by the use of large doses of the bromides; that the dose ordinarily recommended (ten to fifteen drops of fluid extract) is too small, and that as much as a teaspoonful or more four times daily is often needed to secure results.

In the Neurologische Centralblatt for November 7, 1893, Flechsig announced that he had obtained brilliant results in epilepsy by prefacing the bromide treatment with the administration of opium in ascending doses. His method consists in exhibiting the extract of opium for six weeks, in doses rapidly increased from a quarter of a grain to five grains three times a day, when the drug is stopped, and half a drachm of potassium bromide, three times a day, is substituted; these large doses are continued for one month, and then gradually diminished until about thirty grains per day are being taken. This treatment has been quite thoroughly tested, and most observers agree that it is of decided advantage in certain cases.

Collins, who employed it in fifty cases, has drawn the folllowing conclusions: The plan suggested by Flechsig is not a specific in the treatment of epilepsy; in almost every case in which this plan of treatment has been tried there has been a cessation of the fits for a greater or lesser time; a relapse generallly occurs in a period varying from a few weeks to a few months; the frequency of the fits after the exhibition of the opium is, for the first year at least, lessened more than one-half; the attacks occurring after the relapse are much less severe in character than those that patient has been accustomed to having; this plan of treatment is particularly valuable in ancient and intractable cases; in recent cases of idiopathic epilepsy it cannot be recommended; it is an important adjuvant to the bromide plan as ordinarily employed; the opium acts symptomatically and merely prepares the way for and enhances the activity of the bromides and other therapeutic measures; this treatment permits the use of any other substances known to have a beneficial action in epilepsy.

Sulphonal and trional are unquestionably of value, and occasionally succeed where the bromides fail. While the danger of chronic poisoning precludes their prolonged use, they may be prescribed with advantage when a temporary change of treatment is for any reason desirable.

In a certain proportion of cases the exciting cause of the paroxysms seems to be the absorption of toxic substances from the intestine, resulting from indigestion and constipation. This factor of auto-intoxication has been carefully studied by Herter and Smith (New York Medical Journal, August and September, 1892), who studied 238 specimens of urine from thirty-one epileptics. In 72 per cent. of the observations there was an unmistakable evidence of intestinal putrefaction, as indicated by the presence of ethereal sulphates in the urine in large quantities, just before the occurrence of the paroxysm. Such cases are remarkably benefited by regulation of diet, the use of alkaline laxatives, and a course of intestinal antiseptics.

As regards the diet, recent investigations confirm the belief that most epileptics do best on a diet that is chiefly vegetable. Merson (Dietetic and Hygienic Gazette, March, 1892) has recorded the results of a series of observations on twenty-four chronic epileptics. Twelve of the patients were put on nitrogenous and the other twelve on farinaceous food, and this arrangement was continued for four weeks; when the diet was reversed, and observations continued for another period of four weeks. Out of the twenty-four cases there was, in fourteen, a decided decrease in the number of fits during the period of farinaceous diet; the average number of seizures for the farinaceous period was 10.7 as compared with 28.3 for the nitrogenous period. The same results were obtained by Short (British Medical Journal, May 18, 1895) in the treatment of fifty epileptic women who had previously been carefully observed for twelve months in order to ascertain the average number of fits.

The hope that surgical intervention might prove of great value in epilepsy has not been realized, and a more careful study of the cases subjected to operation has proved that this method of treatment has but a very limited field of usefulness. The two classes of cases in which operations have been performed are those resulting from reflex irritation and those dependent upon some focal brain lesion. It has been shown, however, that reflex epilepsy is very rare, the proportion of cases resulting from some peripheral irritation not exceeding one or two in a thousand; and that, moreover, there are very few cases recorded in which the removal of a supposed local irritation has succeeded in permanently checking the convulsions. In the few cases of epilepsy resulting from brain injury the epileptic habit has been, as a rule, so firmly established that an operation rarely cures, and only seldom diminishes the frequency of the attacks.

Mason (Medical News, March 21, 1896) has tabulated seventy cases of trephining for epilepsy, and of this number three died from the operation, three were cured, and four were improved. If trephining and ablation of morbid tissue were reserved for cases in which the injury was received only a few months prior to the development of the epilepsy, the proportion of cures would doubtless be larger than is indicated in Mason's statistics.—Editorial, Union Medical Magazine.

AN AGREEABLE HYPNOTIC.

In many nervous affections attended with insomnia, the physician's efforts to secure the beneficial sleep, are frustrated by the abhorrence exhibited by the patients towards the majority of hypnotics. It is, therefore, extremely important in these cases to select a drug which will be palatable and which, if necessary, can be administered without the knowledge of the patient. In reviewing the list of hypnotics in common use, we find that but few of them are possessed of this quality. In view of its freedom from taste and odor, sulfonal has been found particularly useful in cases where the patient manifested a repugnance towards hypnotics. It can be readily administered in warm milk or other fluids, and never excites gastric or intestinal disturbance. The sleep produced is free from narcosis, being deep and refreshing, and if the drug had been properly administered in abundance of warm fluid there are practically no after-effects. Sulfonal is so safe a remedy that it has been extensively used in pædiatric practice, and here also its palatability is considered a great advantage in its administration.

AFTER-TREATMENT OF OPERATIONS UPON THE NOSE.

In the after-treatment of cases of nasal surgery the employment of antiseptic powders plays an important part. Although it is not generally possible to secure perfect asepsis of the nasal mucous membrane, we can approximate most closely to it by covering its surface with an antiseptic coating which will not readily be washed away by secretions which will exhibit the growth of micro-organisms and thus prevent inflammation and sepsis, and finally, which will relieve pain and other discomfort. There is no better remedy for this purpose than aristol, which, according to many rhinologists, forms an ideal antiseptic dusting powder. "It adheres closely to abraded surfaces, when insufflated, acting as a protective and cicatrisant, and it deodorizes most effectually, without substituting any odor of its own" (Dr. C. N. Cox, Brooklyn Medical Journal). It is especially valuable as a dressing, after operations for hypertrophy of the turbinated, abnormalities of the nasal septum, removal of polypi, etc. Dr. P. H. Anderson (New York Medical Journal) states that when insufflated upon cut surfaces aristol acts not only as an antiseptic, but adheres firmly and prevents the constant oozing of blood and serum from the wound. Furthermore, it reduces to a minimum any reaction occurring as a result of the operations.

MISCELLANEOUS.

CIRCULAR OF INFORMATION REGARDING THE WORK AND THE PRODUCTS OF THE BACTERIOLOGICAL AND VACCINE LABORATORIES OF THE NEW YORK CITY HEALTH DEPARTMENT.

The following circular is published for the general information of physicians in regard to the work of the Bacteriological and Vaccine Laboratories of the New York City Health Department, and the products issued from these laboratories.

BACTERIOLOGICAL EXAMINATIONS FOR THE DIAGNOSIS OF INFECTIOUS DISEASES.

1. Diphtheria.—Bacteriological examination of cultures from cases of suspected diphtheria are made daily (Sundays and holidays included) at the Diagnosis Bacteriological Laboratory. All cultures received during any given day are examined on the morning of the following day, and the results of examinations of primary cultures (those made for diagnosis) are reported by mail to the attending physician before 1 P. M. The results of examinations of later or secondary cultures (those made during or after convalescence) are reported by mail on the afternoon of the day of examination, with the exception of Sundays and holidays, when they are reported on the morning of the following day. In all cases reports may be obtained more promptly by telephoning to the office of the division (telephone 448 Franklin) after 11 A. M., or they will be telegraphed, at the expense of the attending physician, upon his request.

Directions for making cultures will be found upon the backs of the blanks accompanying culture tubes, and must be carefully followed to insure satisfactory results. The information requested on the blanks should be given in full in every case.

2. Tuberculosis.—Specimens of sputum from cases of suspected tuberculosis (other than those occurring in institutions or hospitals) are examined bacteriologically, without charge, and results reported as promptly as is consistent with careful and accurate work, provided that the name and address of the patient be furnished with the specimen for record. Physicians are specially requested to fully fill out the blanks accompanying the bottles furnished by the department for the collection of specimens of sputum. The data so received are solely for registration, and the premises occupied by the patient will not be visited by inspectors of the department, except upon the request of the attending physician, it being assumed that the latter will give instructions as to the proper care of the expectoration and the prevention of infection therefrom.

It is specially requested, in cases where for any reason the bottles furnished by the department are not used in forwarding specimens of sputum, and others are employed, that these be clean and well corked.

3. Typhoid Fever.—The serum-test for the diagnosis of typhoid fever will be applied daily, Sundays and holidays excepted, to specimens forwarded from cases of suspected typhoid fever. Specimens may be mailed to the department in the addressed envelopes provided for the purpose,or left at any of the Department stations,a list of which is furnished below. The directions for preparing specimens, supplied with each "outfit," should be carefully followed. Physicians are requested to furnish in full the data asked for on the blanks supplied for the purpose, and especially to fill out the temperature charts printed on the back of each blank.

4. Rabies.—Animal tests are made, free of charge, for the diagnosis of rabies in dogs suspected of having this disease, when these have bitten human beings. The body of the suspected animal should be forwarded as promptly as possible after death to the Hospital Bacteriological Laboratory, foot of East Sixteenth street, and should be accompanied by a full statement of all the facts regarding the clinical symptoms, etc. These tests require about sixteen days.

GENERAL NOTE.

It is the earnest desire of the Department that the bacteriological service be made as perfect as possible, and as useful to physicians as it can be made.

When, therefore, the bacteriological results do not harmonize with the clinical history, or when there are any defects in or reasons for complaint regarding the service in any respect, physicians are earnestly requested to report these facts promptly to the Director of the Bacteriological Laboratories. Knowledge of defects in the service must reach the Department largely through such reports, and the service can only thus be improved and perfected.

LABORATORY PRODUCTS.

1. Diphtheria Antitoxin.—Antitoxic serum for the treatment of diphtheria is furnished by the department in the following grades and amounts:

Grade No. 3.—Each vial (about 10 c.c.) contains 1,500 antitoxin units (Behring)	$1.50
Grade No. 3a.—Each vial (about 5 c.c.) contains 750 antitoxin units (Behring)	.75
Grade No. 4.—Each vial (about 5 c.c.) contains 1,000 antitoxin units (Behring)	1.25
Grade No. 5.—Each vial (about 5 c.c.) contains 1,500 antitoxin units (Behring)	2.00
Grade No. 6.—Each vial (about 5 c.c.) contains 2,000 antitoxin units (Behring)	3.00
Grade No. 6a.—Each vial (about 8 c.c.) contains 3,000 antitoxin units (Behring)	4.50
Grade No. 7.—Each vial (about 3 c.c.) contains 1,500 antitoxin units (Behring)	3.00

If kept in a cool, dark place (ice box or cellar) and unopened, these preparations will preserve their strength and quality unimpaired for at least six months. The more concentrated preparations are the more desirable, as relatively small amounts of serum are then used. Full directions for use accompany each vial.

2. Tetanus Antitoxin.—Antitoxin serum for the treatment of tetanus is furnished by the Department, as follows:

In vials containing 20 c. c.	$3.00

The immunizing power of this serum is at least 1 to 50,000,000, i. e., 1 c. c. of serum protects fifty million grammes of guinea pigs in weight from a fatal dose of tetanus toxin. The average initial dose of the serum varies with the age of the patient, the gravity of the case and the time when treatment is begun. Full directions for use accompany each vial.

The exact value of tetanus antitoxin in the treatment of this disease, and the best method of administration, have not been fully determined, and the Department especially requests that all physicians using this preparation of antitoxic serum will forward to the Department a full report of the case and the results of treatment.

3. Bovine Vaccine Virus.—The vaccine virus now prepared and issued from the Vaccine Laboratory of the Department possesses great activity and durability. It is issued in the form of a liquid glycerinated pulp, as follows:

Capillary tubes (1 Vaccination)	$0.10
Small vials (10 Vaccinations)	75
Large vials (50 Vaccinations)	3.00

The virus dried on ivory point or quills, formerly prepared and issued by the Department, has been found to be relatively unreliable, and is, therefore, no longer produced.

Before the virus from any animal is used, the calf from which it was obtained is killed and autopsied, and the organs are examined for any evidence of disease. One sample of the virus is then examined bacteriologically, and a second is forwarded to the clinical tester of vaccine virus, and no virus is issued unless the reports of the pathologist, bacteriologist and clinical tester are all satisfactory.

Most satisfactory results have been obtained from the virus subjected to these rigid tests.

The virus should be kept unopened in a cool and dark place until used.

Full directions for use accompany each capillary tube or vial.

4. Mallein.—The Department produces mallein for use in the diagnosis of glanded horses. It is prepared by Nocard's method, and is furnished as follows:

In vials containing 2 1-2 c. c.	$0.35

Mallein should be kept in a cool and dark place until used. Directions for use accompany each vial.

The Department especially requests that all veterinary surgeons who use mallein will forward full reports of the results obtained.

5. Tuberculin.—This substance, prepared after Koch's method, and designed especially for use in the diagnosis of tuberculosis in cattle, is furnished by the Department as follows:

In vials containing 1 c. c. crude tuberculin (2 to 4 doses).........	$0.50
In vials containing 10 c. c. diluted tuberculin, ready for use (2 to 4 doses)	50

The diluted tuberculin is found to lose strength more or less rapidly with time, and is therefore not kept in stock, but is prepared only upon order. Both the crude and diluted preparations should be kept in a cool and dark place until used.

The laboratory products enumerated above are furnished free of charge to all public institutions (hospitals, dispensaries, etc.) throughout the city. They may be procured by physicians at the offices of the Department at any time, day or night. From 9 A. M. to 4 P. M. on any day, except Sunday and holidays, application should be made to the office of the Division of Pathology and Bacteriology. On the latter days, and from 4 P. M. to 9 A. M., application should be made to the office of the Division of Contagious Diseases, Criminal Court Building, which is always open. Diphtheria antitoxin may in addition be purchased at a number of stations throughout the city, a list of which is given below.

FREE USE AND ADMINISTRATION OF DIPHTHERIA ANTITOXIN.

All the grades of diphtheria antitoxin produced by this Department may be procured free of charge at the offices of the Department, or at any of the stations, for use in cases of diphtheria among very poor people, to whom payment for the same would be a hardship, upon the condition that the attending physician will furnish a history of the case to the Department, upon its termination either by death or recovery. Blanks for reporting such cases may be obtained at any of the stations, and should be procured by the attending physician with the antitoxin. The stub attached to the blank should be retained by the druggist, as a voucher for the antitoxin delivered free.

Antitoxin will be administered free to any case of diphtheria occurring in the city, upon the request of the attending physician. Such requests should be made to the office of the Division of Pathology and Bacteriology (telephone, 448 Franklin) from 9 A. M. to 4 P. M. daily, with the exception of Sundays and holidays. On these latter days, and from 4 P. M. to 9 A. M. daily, requests should be made to the office of the Division of Contagious Diseases (telephone 450 Franklin), which is always open. The medical inspectors detailed for the free administration of antitoxin are on duty at all times, day and night, and requests for their services will receive prompt attention.

NEWS AND NOTES.

THE REFINING INFLUENCE OF THE ANTI-VIVISECTION AGITATION.

Last year, the Marine Hospital Service was called upon to test the value of the cobra venom as a remedy against cholera, it being claimed to be an infallible remedy long used among the mountain tribes of India. Surgeon Kinyoun was detailed to make some experiment with it on animals, and, this fact being mentioned in the daily papers, he began to receive anonymous letters, two of which he publishes in the report of the supervising Surgeon-General for the year 1896. We quote one of them. They are enough alike, to have come from the same source, and we do not like to believe that there are many who would indulge in such malevolence. They make interesting reading, in connection with the familiar charge that experiments upon the lower animals tend to destroy the sympathy and benevolence of the experimenter.

One is naturally led to wonder what is the connection between the moral and intellectual condition of the writer in the following communication and the anti-vivisection agitation. Is it one of cause and effect? If so, which is cause, and which is effect? Does the persistent indulgence in misrepresentation and vilification develop this form of moral monstrosity? Or, does this peculiar form of monstrosity find the anti-vivisection movement an attractive and congenial opportunity for the display of its characteristics?

"You vile, merciless, rascally fiend! I judge from your name, you are a nasty Frenchman, with no heart and without a God. If you think you will be permitted to set up a place of torture in this country for poor animals, you are vastly mistaken. You will have to betake yourself to the vile and Godless country from which you came, or to the hell to which you are destined; and if you wish to indulge in such fiendish performances when you reach the latter place, which I hope you will soon, unless you repent, you will have an opportunity, with kindred fiends, of torturing yourself through the endless ages of eternity. I am a woman, who despises brutes and all brutal actions."

It might be hard for a "vivisectionist" to believe in the genuineness of such letters, to believe that any man or woman would freely indulge such vindictive feeling toward any living being, even a fellow-man; but, unhappily, expressions of similar malignity have so often found their way into print, over the signatures of anti-vivisection agitators, that the wish to intimidate or to annoy an investigator seems the most plausible explanation of such anonymous attacks.

It must not be supposed that even the most persecuted worker in experimental medicine believes that all anti-vivisectionists cultivate this kind of feeling, or would descend to such coarse expression of it. But can we not properly ask those who give the anti-vivisection movement respectability, to carefully consider how far such utterances reveal love of brutes, with willingness to undergo self-sacrifice for them; and how far they represent love of one's own opinions, and associations, and purposes, with willingness to sacrifice somebody else to them; and hatred of any one who would prevent this latter sacrifice?—Editorial, Philadelphia Polyclinic.

AFTERMATH OF THE AMERICAN MEDICAL ASSOCIATION.

The gathering of several thousand members of any profession leaves an impression which cannot altogether be accounted for by the transactions of the men formallly assembled. The spirit of the American Medical Association is in itself an interesting development of a democratic country. The influence of the Association is tremendous, yet it may be said to have no standard of membership, and no specific object. In attempting to analyze the paradox, failure is inevitable, as it is always when we attempt to put into words the rationale of a large concourse of human beings.

An association of medical men could scarcely be effected on broader grounds. Every physician in good standing in the regular profession is eligible to membership on complying with conditions so simple as to amount to little more than registration and payment of dues.

The aims of the Association are the advancement of the welfare of the average physician, scientifically and otherwise. In consistently carrying out such a broad policy, mediocrity cannot always be avoided, but the results, from the standpoint of the average man for whom the Association exists, is satisfactory and dignified.

There is only one point in the policy of the American Medical Association which The Reporter would criticise adversely, and even this disagreement is one of method, not of principle. We regard the attempt to codify any strictly ethical or moral matter as utterly futile. Law should be the attempt, not to make a bad man good, but to protect society from the overt acts of the bad man. A written or printed code of ethics can never be so accurately worded as to restrain a shyster nor to guide a man who wishes to conform to the dictum of his profession, but who lacks the instinct of good taste and propriety. On the other hand, the man who could satisfactorily use a code, needs no such assistance.

The Association has never been so lavishly entertained as at Philadelphia, and we believe that a local contemporary has voiced the sentiment of the great majority of the local profession in saying that the city had no axe to grind, no candidate for office, no favorite meeting-place, no pet scheme of management. Yet we have to regret, and, in no sense of responsibility, to apologize for, the attempt of a few men to make political capital out of what should have been the purest hospitality.

One thing could not but impress the most impartial observer at the various luncheons and other entertainments of the American Medical Association. Although the dues paid by members, even if applied in such a way, did not begin to represent the value of the entertainment offered, not a few of the guests and some of the ladies accompanying them, acted as if they were at a bargain counter, elbowing their neighbors for a first serving or a choice bit of food, or complaining of some commission or omission on the part of their entertainers. Yet after all, such criticism was deserved by the scantiest minority of those in attendance, and, for the most part, one could well be proud of belonging to an association which makes no pretense to being select.

One of the noteworthy features of the American Medical Association is the number of satellite societies which its mass has attracted. The medical editors, the examining boards, the colleges, were all represented by independent societies, and the Academy of Medicine held an important two days' session before the American Medical Association assembled. It is rather a joke on the members of the last-named organization, that, with all their college education, a glaring error in grammatical construction, should have occurred on the first page of their announcement.

Nothing could have been more timely than the recent plea of The Reporter for greater care in the preparation of programs. Some of the sections published lists of papers three times as long as could have been presented in the utmost allowance of time at their disposal. For the man intending to devote his attention to one section, this meant no greater inconvenience than disappointment at not hearing some papers in which his interest had been aroused and fatigue at having to listen to some which were utterly unworthy of an audience. It was quite impossible for one whose time was divided to make plans to hear any particular discussion or paper. It was openly charged by many in attendance that some of the papers had not even been written, their authors (?) merely wishing to see their names on the program.

Different men afforded the most diverse examples of methods. Some sacrificed all personal considerations in order to save the time of the section and allow other papers to be presented. Others gesticulated over platitudes, without interest to any progressive physician, not only without regard to courtesy, but even in defiance of time limits set by joint agreement. We would repeat the recommendation that every society should have an editorial committee which should reject, curtail or abstract every paper not of suitable length and interest, with the same impartiality as if it were presented for publication.

And yet, with all the opportunities for criticism, what a grand education it is to meet with one's professional associaties of many latitudes and longitudes, of both sexes and all kinds! And, with all our shortcomings as a profession, how much there is to make us feel the benefits of mutual support and to give us a modest but enthusiastic pride in being American physicians!—Editorial, The Medical and Surgical Reporter.

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REVIEWS.

A HAND BOOK OF CLIMATOLOGY.

Embodying Its Principles and Therapeutic Application, with Scientific Data of the Chief Health Resorts of the World. By E. Edwin Solly, M.D., M.R.C.S. Lea Brothers & Co., Philadelphia, 1897.

The author introduces his subject by saying that the "non-existence of a systematic treatise on medical climatology, and the fact that this subject has engaged the author's attention for thirty years, may be advanced as the reasons for the preparation of this volume. The time, moreover, seems ripe for such an endeavor, for there is a growing appreciation both on the part of the profession and the public at large, as to the value of climate in the prevention and treatment of disease, and fortunately to answer this awakening we have now accumulated accurate and extended meteorological observations upon which to found natural laws affording trustworthy conclusions."

The last few years has certainly been productive of a number of papers on the value of climate for the treatment of disease, and we are not so sure that the author is entirely accurate in saying that no systematic treatise has heretofore appeared. On the other hand, we most heartily accord to the one which he has offered us, a position of honor, and we cannot fail to recognize that he makes valuable contributions, and in a practical way to this phase of medicine. At least for one disease climate has always outlived every other form of treatment. We mean phthisis, and it is a very well known fact that phthisis in its earlier stages is entirely amenable to climatic treatment.

The work is divided into three sections and each section into a number of chapters. A statement of the captions of these chapters will give an idea of the way in which the author considers his subject, even if he does not give anything like an adequate idea of the detail and pains that he takes in presenting his facts.

Chapter I is entitled, "Principles of Medical Climatology." Then follow chapters on physiology, ethnology, geographical distribution of fideases, and classification of climates. This completes Section 1.

The next section has chapters on Phthisis, Form of Phthisis as Influenced by Climate, the Results of the Treatment of Phthisis by Change of Climate, and lastly, Forms of Disease other than Phthisis as Influenced by Climate. Under this letter head Asthma and Hay Fever which the author calls Nasal Asthma receive some attention, as well as some heart troubles and nervous disorders.

The next section is for the most part geographical and gives the climate and the conditions, as well as neat and accurate maps of a variety of regions—North America, Mexico, South America, Europe, Africa, Australasia, and some of the islands. Naturally the climates of North America receive the lion's share of attention. The name of the publishers is a sufficient guarantee of the character of their part of the work. The volume is unquestionably a desirable one and useful. We wish it the success it deserves.

The Menopause. By Andrew F. Currier, A.B. M.D. New York, D. Appleton & Co., 1897.

In the embellishment of the title the author gives the key-note to the work.

"A consideration of the phenomena which occur to women at the close of the child-bearing period, with incidental allusions to their relationship to menstruation, also a particular consideration of the premature (especially the artificial) menopause."

The author is already well known to the American public through his many papers on gynæcological subjects, and the presentation by him of a systematic treatise on one of the phases of woman's life is both welcome and expected. It has been a long time, he tells us, since we have had an English work, or even a translation of a foreign work on this subject, and there has unquestionably been such a vast amount of work done in operative gynæcology during these years, that something ought to have been added to our knowledge of the menopause.

"The writer would beg leave to say that he has had the subject of this work under consideration many years. He was never able to see the sense nor the logic of the traditional teaching repeated generation after generation that the menopause was a serious, yea, a most dangerous time and experience. That the matron and the maid alike should approach it with fear and awe; that if this dangerous Rubicon were once past, joy and felicity would wait upon the remaining years of life

"Another serious error which it is the writer's desire to controvert, is the prevalent one that there is in some way the most intimate relationship between cancer, especially cancer of the womb and the breast, and the menopause."

The treatment of the abnormal conditions associated with the menopause receive full consideration, and the little volume sustains the reputation long since earned by the author for thoroughness, conscientiousness and practical value in his writings.

Hysteria And Certain Allied Conditions By George J Preston, M. D., Professor of Diseases of the Nervous System, College of Physicians and Surgeons, Baltimore. Philadelphia, B. Blakiston, Son & Co., 1,012 Walnut street, 1897.

Again we are confronted by the statement that there is no recent book on the subject as the excuse for the appearance of this little volume.

It is surprising how many excellent medical men are puzzled and perplexed by hysteria in its manifold forms. The fact that some of the worst cases of Hysteria are seen in males, often fails to enter into a medical man's consideration. Its continual appearance among females is apt to put one more or less on guard, but even with them it sometimes assumes such a complete disguise that it will baffle the most careful consideration of experts.

The subject is always an interesting one, and any separate treatise on it must present some new fields for thought, and in the present case the author's suggestions as to prophylaxis, as well as his detailed statement of the treatment is more especially the result of personal experience, and this is always the most interesting part of a man's writing.

Hydrotherapy, electrotherapy, massage, the rest cure, hypnotism and surgical interferences are the methods of treatment that are separately considered.

Eye-Strain in Health and Disease. With Special Reference to the Amelioration or Cure of Chronic Nervous Derangements Without the Aid of Drugs. By Ambrose L. Ranney, A.M., M.D., author of "Lectures on Nervous Diseases," "The Applied Anatomy of the Nervous System," etc., etc.; late Professor of Nervous Diseases in the Medical Department of the University of Vermont and of the Anatomy of the Nervous System in the New York Post-Graduate Medical School, etc. Illustrated with 38 wood-cuts. One volume, royal octavo, pages viii-321. Extra cloth, beveled edges, $2.00, net. The F. A. Davis Co., Publishers, 1,914 and 1,916 Cherry street, Philadelphia; 117 W. Forty-second street, New York; 9 Lakeside Building, Chicago.

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EDITORIALS.

GENERAL SUPPURATIVE PERITONITIS TREATED BY THE NEW METHOD

In the Johns Hopkins Hospital Bulletin for July, 1897, Dr. J. M. T. Finney, Associated Professor of Surgery at the Johns Hopkins University, gives a record of five successful cases of General Suppurative Peritonitis—he does not say how many unsuccessful cases, but the presumption is that there were none, except the one added to the paper after it had been completed. In view of the fact that several of the best known operators in the country have declared that they have never seen recovery by any form of treatment in this most serious condition, and in view of the fact that several have considered a single recovery in a great many cases to have been marvelous, and only to have occurred after the introduction of serum therapy, it is no less satisfactory than amazing to find one operator with so many successful cases, and we must give our readers the benefit of his new method. Without going into the causes that induced him to follow the method, we will give a statement of the principal points in regard to it in his own language:

"The principle involved is not a new one, only in the manner of carrying it out is there any originality claimed. The steps of the operation are as follows:

"Make a sufficiently long incision to admit of easy access to all parts of the peritoneal cavity. Quickly withdraw the coils of small intestine from the peritoneal cavity, beginning with the worst coils first. Remove all, or as much as is necessary of the small intestine and place it outside of the abdomen, covering with warm gauze or towels, thus practically disembowelling the patient for the time being. Then thoroughly and systematically wipe out the peritoneal cavity with large pledgets of gauze wrung out of hot salt solution, paying particular attention to the pelvic portion. In some cases it may be well, in addition, to flush out the cavity with the warm salt solution, but this is rarely necessary. Next the small intestine should be systematically examined, loop by loop, while still outside the abdomen, and rendering it microscopically clean by wiping with gauze compresses wrung out of hot salt solution. It is necessary to wipe with considerable force at times, in order to remove adherent flakes of partly organized lymph. It should be done thoroughly and conscientiously, however, as upon this depends, we believe in great measure, the success of the operation. It facilitates the cleansing process as well as lessens the shock of the operation, if the wiping of the intestinal coil is carried on under a constant irrigation of warm salt solution. After being cleansed microscopically of all foreign material, pus, fæces, lymph, and so on, the intestine should be replaced in the abdomen, the worst or sutured coil being the last or most superficial, in order that it may be the better drained by being packed about with gauze, if necessary. The abdominal wound is then tightly closed, leaving just room enough between two sutures for the gauze drain. If there are any evidences of distension or pain, the abdomen should have the Pacquelin cautery thoroughly applied, and the bowels moved early by calomel in broken doses, followed by salts, and a turpentine enema."

We have given the detail of the author's treatment because we believe that it is owing to the minute attention of every little point, that one man will be successful where many fail. We would suggest that the method is certainly most rational, and the results in even the few cases reported, would justify its trial in any case of general suppurative peritonitis that might fall into our hands.

A DEPARTMENT OF PUBLIC HEALTH.

We quote from the American Medico-Surgical Bulletin for July, 1897, an editorial with this title, because it gives in a succinct way a great many useful facts about a much needed public institution. Nothing is said about the existence for a time of the National Board of Health, nor of the immense value that it proved itself to be, even in the short period of its existence, but the continued agitation by the Journals, some of which were not over-enthusiastic in their endorsements of the idea when it was originally proposed, and up to the point of existence successful, will possibly bring about a re-establishment of a similar Board, and with it the benefits which must accrue.

"All great interests of the nation, wherein it has relationship with other powers or in which interstate rights are involved, should and must eventually have distinct representation among the Cabinet officers at Washington. Already most of these interests are so cared for. General State matters are in the hands of the Secretary of State. Monetary affairs are looked after by the Secretary of the Treasury, the army and its interests are cared for by the Secretary of War, the Navy has its Secretary, the Post Office is remembered in the same manner, such internal affairs as pensions, patents, education, the census, the Indians and Public Documents have their Secretary of the Interior, legal matters are looked after by the Secretary of Justice and Agricultural Interests are looked after by a Secretary of Agriculture. Though health and life are of more importance than all the rest there is no Secretary of Health. This is wholly due to the fact that medical men lack public spirit and do not look properly after their own interests. They now have an opportunity to redeem themselves in this by rallying to the support of Senator Mallory, who is seeking to have established a Department of Public Health. Every medical man in the United States should take an interest in this effort and try to further it. It is meeting with some opposition from the public press, but this can easily be stopped if a large proportion of the medical men in each community will use their influence with the editors and their friends. The Philadelphia "Public Ledger," of July 9, opposes Senator Mallory on what we deem rather strange grounds. Speaking of his proposed new department the "Ledger" says that it 'seems to be less needed now than at any previous time, and likely to be required less as time rolls on, medical science improves, and popular interest in sanitation increases. Years ago, when cholera, yellow fever, and smallpox were frequent visitors and other epidemics were constantly feared, there might have been some wisdom in creating a national department of hygiene, with the necessary authority to protect the people against disease; but we got along without it then, and we can do so much more easily now, when quarantine surveillance is so efficient that it is almost impossible for an epidemic to be imported, and when municipal and private sanitation is so well observed that such diseases could scarcely find lodgment if they did come into port. In a word, all the usefulness that could be accomplished by another cabinet officer in this respect appears to be very well done already by existing appliances and regulations, and therefore there seems to be no reason for burdening the Treasury with the expense of another administrative department, with its inevitable army of clerks and other employés.' Medical science improves as knowledge increases. It is the increase in knowledge that has brought about the improved sanitary conditions to which the "Ledger" refers. A Department of Public Health trying to arrest the progress of an epidemic without a knowledge of bacteria would be an amusing spectacle. In the past we had no use for such a department because it would not have known what to do. Anything that it might have tried to do would have been just as likely to do harm as good. The editor of the "Ledger" might well have said that we got along better instead of just as well without it. Concerning his claim that all the usefulness that could be accomplished by another Cabinet officer appears to be well done already, we can only say that it may appear so to laymen who lack data on which to reason, but it does not to medical men who have made a study of the subject. Before the days of railroad trains, sewing machines, telegraphs, steamboats, harvesters, and all other modern improvements, there were multitudes of people that thought the same of them. They believed that the work was well done without them. No one thinks so now, because everybody has had a demonstration to the contrary. At present medical men have but a faint conception of the extensive field of usefulness that would surely open up for such an officer to do. State and municipal boards of health are at present usurping the functions of such a department in a way that is likely yet to be challenged as to its constitutionality. Goods consigned to one State, are fumigated and damaged in another while in transit. Some day an over-zealous functionary may start the question of the utility of his work and the question of the right of his State to do such work be challenged. As population increases and factories of various kinds multiply along streams questions regarding the pollution of such streams are sure to arise. Where they pass through two or more States, and where many cities take their water-supply therefrom the question as to who is responsible for such pollution is sure to arise. The State that refuses to enforce proper sanitary measures will thus be a menace to the States below it on the stream, and without a Department of Public Health there will be no proper authority to refer such disputes to for proper adjustment. States are ever sure to impose on States in sanitary matters just as individuals impose on individuals, in the same direction. When epidemics of disease threaten us from abroad we seldom have intelligent co-operation among all the State boards of health in fighting them back. The editor of the "Ledger" is too certain of the prowess of these bodies. Some day he may awake to a full realization of their lack of efficiency because of their divided interests. When we find the health officers of one port seriously accusing those of another of having sold themselves to a shipping company to violate the quarantine laws and endanger the country with yellow fever, we are likely to begin to doubt their efficiency. As long as we are menaced with diseases from abroad and know how to protect ourselves from such diseases, so long should our protection be the best that the Government can supply. In the not distant future we are likely to undertake the extinction of some of our most contagious diseases by preventing their being spread. The State that starts so meritorious a reform is likely to have its plans thwarted by neighboring States whose citizens take no interest in such work. They will continue sowing the seeds of disease again as fast as extermination goes on.

Bacteria have no more respect for State lines than has thistle-down. As it would be useless for Ohio to attempt to exterminate the Canada thistle without the action of a National Department of Agriculture, so it would be equally useless for the same State to try to exterminate scarlet fever without the aid of a National Health Board.

Only by having a department of Public Health could so desirable a measure hope for success. It could protect each State against the transmission of contagion from all other States and against foreign nations. The grand work of sanitary science has scarcely begun and nothing could foster and advance it so much as a Department of Public Health. If the study of disease as related to animals is worth supporting by the Government, then the study of disease as related to man is certainly by the Government, then the study of disease as related to man is certainly of equal importance. If Dr. Salmon can do such good work as he has done in the last fifteen years with a Bureau of Animal Industry, we certainly have other medical men who could do equally as well in a Department of Public Health."

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ORIGINAL ARTICLES.

THE KNOWLEDGE OF HEART DISEASES IN THE XVIII CENTURY.*

By MORRIS MANGES, A.M., M.D.,
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In choosing this subject for my paper I was actuated by two motives: (1) To give a general, although brief, sketch of what was known of diseases of the heart in "pre-stethoscope" times, and especially in the early part of the XVIII century; (2) as a corollary to this, to recall how much may be accomplished by means of well-directed observation and a correct appreciation of manifest and simple signs. In our time, when instruments of precision are so extensively employed; when percussion, the various stethoscopes, sgphymographs, cardiographs, etc., are at our disposal; when general laws of hydrostatics and dynamics have been called upon to explain the various phenomena of the circulation, we naturally lay much less stress upon the many minor features which alone afforded diagnostic indications to the physicians to whom these indispensable aids were unknown. Far be it from my purpose to underrate in any way the valuable results which have thus been obtained; yet it cannot be gainsaid that in the general diagnosis and treatment of cardiac diseases we rely entirely too much upon our modern instruments of precision, and are thus led to treat not patients, but circulatory conditions.

It is erroneous to suppose that but little was known of cardiac disorders before the discovery of percussion and mediate auscultation. The anatomy of the heart was well known to the ancients, but organic diseases were not recognized till the XVI century, and then only at autopsies. This ignorance was due to the Hippocratic doctrine that the heart could not become diseased, inasmuch as it was the source of animal heat and the seat of the soul. In 1628 Harvey announced his views on the circulation; Auerbrugger discovered percussion in 1761;

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* Read before Metropolitan Medical Society of New York City.

while Laennec's treatise on auscultation did not appear till 1821. Nevertheless a wonderful amount of good work was accomplished in the pathology, diagnosis and treatment of cardiac diseases by such men as Vieussens, Lancisi, Morgagni, Albertini, Senac and others who lived in the earlier part of the XVIII century. Their work it was which laid the foundation of all our knowledge of these lesions. Even Laennec admits the great importance of their investigations.

That so much should have been accomplished is all the more remarkable when we consider the state of medical science of this period. Thus Vieussens believed that the circulation of the blood was maintained by the continuous explosions in the heart and blood vessels which resulted from the combustion of the saline and sulphurous particles in the blood with the nitrates of the air and the vital spirits. Dyspnœa was believed to be always due to hydrothorax; asthma was only a nervous disturbance. Polypi in the heart (ante- and post-mortem clots) were for a very long time considered the cause of many protracted diseases.

In passing, it may be of some interest to take a hasty glance at the condition of the medical profession in the early part of the last century. It was an age of transition in which observation and speculation each played a great part. How far reaching were some of the theories promulgated during this period may readily be appreciated by reading Virchow's Croonian lecture.* No less than eighteen great systems of medicine were formulated, ranging from the eclecticism of Boerhaave down to the realism of Bichat, each in turn usurping a temporary supremacy in the medical thought of the age. The rage for theory was a sign of the times, an endeavor to assimilate the many facts which careful observation revealed and which the existing state of knowledge could not explain. And yet it was a curious age of contradiction, where superstition prevailed to no small degree. Witches were burned as late as 1782. Baas states that an ordinance of the Berlin authorities in 1704 required that those who died, even of the plague, without having taken any medicine, should be hanged after death in their coffins. In 1714 the days and hours on which the King of England would cure the King's evil (scrofula) were announced on posters. How terrible was the condition of the insane asylums we all know; but some of the great hospitals were even in a worse state. Single halls in the Hotel Dieu at Paris contained enormous numbers of patients; large beds (5 feet wide) held four to six patients, the feet of one to the head of another. Men, women and children were indiscriminately mingled together; patients with the plague were alongside of a woman in labor. The bodies of the dead usually lay twenty-four hours before they were removed from these large beds! In Spain, the physicians solemnly recommended that the streets ought not to be cleansed, since it would be injurious to disregard the prevailing customs, and furthermore the offensive air was especially good for destroying the materials of infection. The materia medica of this century is scarcely credible, including, as it did, precious stones, dung of various animals, various kinds of lice and vermin, etc. In 1798, Marcus, in treating fevers on the Brunonian principle, used on an average for each patient one drachm of opium, 200 grains of camphor, one ounce of Hoffmann's anodyne, over one ounce of cinchona bark, one pound of rectified spirits; musk, sulphuric ether, etc., being used as well. He, too, fed the fevers! The Prussian medical edict of 1725 was to be applied to physicians, surgeons, apothecaries, grocers, bath keepers, midwives, mountebanks, dentists, students of medicine, old women, shepherds, executioners and water sellers. The exact details may be read in Baas's excellent treatise on the History of Medicine to which and a monograph by Phillipp* I am indebted for many data.

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* British Medical Journal, March 18, 1893.

Passing then to the special knowledge of the diseases of the heart, I shall refer to a few cases of hydropericardium, in which Vieussens was able to make a correct diagnosis during life, as was proved by the autopsy. He first learned to recognize the condition in a boy six years of age. After frankly admitting his inability to make the diagnosis in this case, he speaks of the following patient, with whom he was more successful. His description reads as follows: "The patient was a boy six years old, of a pituous-melancholic temperament, with a pale complexion and a somewhat dull eye; the extremities were cool, the breathing labored, especially after climbing stairs; the pulse soft and rapid. After considering these symptoms I told the boy's father that his son would undoubtedly suffer from an incurable thoracic disorder unless frequent mild diuretic laxatives and gentle resolvents were soon employed. Soon after I was called away to Paris, where I remained a year. On my return I ascertained that the boy had only received occasional doses of wormseed and manna, because the physician in charge had diagnosticated worms. Being called there a second time I visited the boy for three days, morning and evening. The dull pain on the right side, which increased on lying over on the left side; the low fever, the increased flow of saliva, the occasional chills, the superficial respiration, which was very rapid even while sitting up, the obstinate and very annoying cough —all these signs led me to believe that the lobes of the right lung were adherent to the pleura and were indurated; and inasmuch as the child's legs were swollen, as he could not lie with his head low, and as the dyspnœa was increased when he lay on the right side, I concluded that there was fluid in the left pleural cavity. Finally I also diagnosticated the presence of fluid in the pericardial sac, because the disease, both at its beginning and end was accompanied by palpitation of the heart, leaden color of both eyelids and great depression; and because there were several attacks resembling those in the boy with hydropericardium.

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* Berlin, 1856.

"Two other eminent physicians who saw the case pronounced it disease of the liver and chest; however, another whom I called in, corroborated my views.

"At the autopsy the liver and other abdominal organs were found normal. But the left pleural sac was distended with yellow serum; likewise there was an excess of lymphatic fluid in the pericardial sac. The heart was soft and contained no polypi. The left lung was flaccid, but the right was as hard as Roquefort cheese and was so adherent to the pleura that the greatest force was necessary to separate them."

Surely, to-day, we could make no better diagnosis of a case which began as a right pleuro-pneumonia with consecutive induration and pleuritic adhesions. The pleural and pericardial effusions were due to the subsequent cachexia. To any one who may object that this was simply a lucky guess, I would reply that these observers, unlike many of ourselves, always faithfully reported their failures as well as successes.

Lack of time forbids my giving additional cases. In all of them there is the same attention to details, especial stress being laid upon the color of the cheeks, lips and eyelids; the turgescence of the lips, the changes in locomotion, the temperature of the various parts of the body, the posture of the patient and the effects of changes of it on the pulse, respiration and color of the face. Even the mental condition is always noted.

It is also very interesting to read how correctly this same observer not alone described a case of mitral stenosis, but even correctly explained the effects of the lesion upon the heart and the circulation. His statements are true even to-day.

Where so much attention was bestowed upon the pulse it is by no means surprising that the pulse of aortic insufficiency was observed by him; nor did its significance and the manner of its production escape Vieussens. It was quaintly described as follows: "The pulse on both sides was very full, very hard, slightly irregular and very rapid; the artery struck the finger-tip like a tense string which was vibrating with great force. Such a pulse I had never before encountered, and I pray to God that I may never again feel one like it!"

And now to briefly recapitulate what was known on the subject of heart lesions at this period. Of course, the majority of these facts was obtained from post-mortem examination and not from diagnoses during life. But this in no way lessens their importance.

Pathology.—Three classes of lesions were recognized: the structural, mechanical or valvular, and nervous. The relations of the two sides of the heart were clearly understood; it was known that the left side was more frequently hypertrophied, while dilatation was more common on the right. Senac showed that valvular stenosis was found more frequently on the left side.

Morgagni explained how lesions on the right side caused pulmonary stasis and hæmorrhage.

Hypertrophy and dilatation were both included under the term aneurism of heart, the former being called aneurismal enlargement, the latter varicose, a classification which is obviously based upon the differences in the dilatation of arteries and veins. Eccentric hypertrophy was clearly explained by Albertini.

The various mechanical effects of valvular lesions were well known; it was recognized that insufficiency as well as stenosis would interfere with the circulation.

The frequency of pericardial changes in organic diseases was noted, while even inflammatory lesions were recognized by Senac. He asserted that the inner surface of heart was more frequently covered with lymph than the outer, and that this condition was most often found in "epidemic stitches-in-the-side" (a quaint name for pneumonia) and pulmonary and mediastinal diseases. The relations of endocarditis to rheumatism are undoubtedly alluded to in Senac's assertion that the heart itself may be the cause of the fever.

The real nature of heart-clots (so-called polypi of heart) was also, for the first time, made manifest. Morgagni called attention to the rarity of primary hydropericardium; he also proved that when it was secondary it was always associated with general anasarca. Senac even showed that the forcible action of the heart in fevers was not due, as was popularly supposed, to a thickening of the bodily juices, but to disturbances in the innervation of the heart.

Etiology.—In discussing the etiology of aneurism of the heart Lancisi includes: (1) Heredity; (2) mechanical causes; (3) chronic catarrhs (i. e. emphysema); (4) nervous excitement; (5) overexertion, as in orators, players on wind instruments; (6) excesses in venery and eating (syphilis and gout). If to this list were added congenital defects and the inflammatory lesions, it would almost fulfil our modern requirements.

The importance of atheroma was recognized by Senac, to whom also belongs the credit of having first described inflammatory conditions of the heart.

The relations of pericarditis to organic disease were also understood.

Symptomatology may be dismissed, as I have already shown how cases were described and the various symptoms are included in the discussion of the other topics. Their value, too, is lessened by the great change in nomenclature and pathology.

Diagnosis.—I have already alluded to the many minor points upon which Vieussens based the diagnosis of cardiac disorders. The pulse, of which Galen described no less than twenty-six varieties, was a very important factor, so much so that in cold weather physicians were accustomed to carry muffs lest the cold should lessen the delicacy of touch. The small pulse of mitral stenosis, the water-hammer pulse of aortic insufficiency and the hard pulse of hypertrophy were all recognized. Even the pathognomic importance of various pulsations in the neck in aneurism of the right ventricle was clearly set forth and explained by Lancisi.

Hypertrophy and dilatation were distinguished by laying the hand on the pericardium and estimating the force of the heart action; they also noted the site of the apex-beat and the extent of the area over which the heart action was visible. It is difficult to say to what extent these investigators used auscultation and percussion; we know, however, that Lancisi employed percussion on the sternum. Haeser, in discussing the work of Albertini, states that no mention is made of applying the ear to the chest; yet it is scarcely conceivable that this should not have been attempted. Undoubtedly it was soon abandoned, since only well-advanced cases, with their confusing chaos of sounds, came under observation. It is a popular error to suppose that auscultation originated with Laennec. Not so; his discovery consisted in the use of the stethoscope, or mediate auscultation, as stated on the title page of his celebrated treatise. The mucous râle and succussion were both discussed by Hippocrates (460-370 B. C.). The school of Cnidos knew the pleuritic friction sound; this sect flourished 400 B. C. Auscultation is also alluded to in the works of Cælius Aurelianus, who lived in Rome at the end of the IV century, A. D.

The diagnosis of cardiac disorders was strengthened by the presence of shooting pains in the thorax, shoulders, radiating thence to the middle of one or both arms; it might also be present in the scapular region. In addition, the previous occurrence of prolonged mental depression or sudden, violent injuries, carrying of heavy burdens, prolonged marches, syphilis, alcoholism and the playing of wind instruments—these were the guides of Albertini.

It was admitted that it was difficult to recognize varicose aneurisms (dilatation) because of the increased area of impulse and the feebleness of the heart action. The same is true of pericardial adhesions. How pericardial effusions were discovered has already been stated.

The relations of pulmonary œdema, congestion of lungs and hæmoptysis to cardiac lesions were known to Albertini.

Treatment.—This need not detain us long, as the drugs of this era are so opposed to those of our own time; furthermore each physician endeavored to make his treatment correspond to the theories of the school to which he owed allegiance. Thus Albertini, a believer in the mechanical school, taught that the pressure of the fluid elements upon the solids ought to be lessened, and the resistance of the latter ought to be increased; in other words, that the blood pressure ought to be lowered. Accordingly he recommended venesection, low diet, rest in bed, iron preparations, etc.

Naturally venesection played a very prominent part in all treatment at this time, although it was employed even more extensively in the early part of this century. The prevailing idea was to lessen the volume of blood, and in this way to diminish the work of the heart. Its timely use would prevent all organic diseases of heart; yet it was to be avoided when the patient is very weak or when the heart was failing. A small, irregular or rapid pulse did not necessarily contraindicate it, unless the patient was dropsical.

Senac's general indications for treatment were: (1) Venesection; (2) avoidance of overexertion; (3) careful diet and mode of life; (4) regulation of the bowels; (5) regulation of the functions of the stomach and liver—here certain mineral waters were valuable; (6) quieting of the nervous system, especially with Hoffmann's anodyne; (7) no mental excitement; (8) the cure of other diseases, if present, especially hæmorrhoids and worms; (9) treatment of attacks and dropsy (in the latter squills was very valuable).

In attacks of stenocardia it was advised to use hot hand- and foot-baths and cold syringing of chest. Kermes mineral and squills were also employed.

In the intervals, iron waters were employed; prolonged use of milk, whey, etc., were also advised. In aneurism of the vessels and of heart Morgagni advised very low diet and rest in bed for forty days, so that, at the end of the cure, the patient was scarcely able to lift his hand. These methods are employed even now, and with success.

These, then, are a few of the facts which a study of this period have revealed. I greatly regret that lack of time to-night prevents my going into further details on this interesting subject. One is amazed at the penetration and ingenuity of these pioneers in cardiac pathology in interpreting the complex problems set before them. A careful study of their writings will repay any one; indeed in our anxiety to explore unknown fields, we have neglected what has already been accomplished. It is greatly to be regretted that the study of the history of medicine is entirely ignored in all of our colleges.

The above also shows how much well-directed observation can accomplish. How much more ought we do when all our modern means of research are employed as well! The practical conclusion is that, once having established the diagnosis, the heart murmur is of relative unimportance. Our guide, thereafter, is the patient's general condition, and the heart is to be treated only in so far as is necessary for this purpose. And in this lies the secret of why the physicians of the XVIII century, ignorant of all our modern pathology and diagnostics, could do as much as they did.

SUPRAPUBIC CYSTOTOMY FOR STONE.—SOME COMPLICATIONS.*

By VIRGINIUS W. HARRISON, A.M., M.D., Richmond, Va.,
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On May 22, 1897, I did a suprapubic cystotomy for stone on Paul A., white, age 16. After cutting through the abdominal parietes, I found the peritoneum presenting itself in front of the bladder, almost touching the pubic bone. It was some time before I was certain that the peritoneum was far enough out of the field of operation to make the incision into the bladder without doing harm to it, but this was finally accomplished, and without further trouble a small mulberry calculus was removed. A catheter was placed in the bladder, and iodoform gauze loosely packed around it to favor rapid and thorough drainage; and a stitch was taken in the upper angle of the wound, extra gauze being placed here to protect the peritoneum as well as possible. The patient was put to bed in good condition.

The usual preparatory treatment was administered: the bladder being washed out just before the operation, and filled with sterilized water, and the rectal bag was used.

The patient did well until 6 A. M., when he became restless and suffered pain in the abdomen; pulse 98, temperature 101.4°. At 12 M., pulse 110, temperature 103.2°, with all the symptoms of septic infection of the peritoneal cavity, viz.: distended and rigid abdominal muscles, face flushed and anxious, nausea, etc. It was determined to open the abdomen immediately. The bladder was first washed out and the incision which had been made in the bladder at the first operation was closed with interrupted silk sutures; a catheter was placed in the urethra to insure drainage and to prevent the urine from soiling the peritoneum and wound after it had been washed and dressed.

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*Read before the Richmond Academy of Medicine and Surgery, July 15, 1897.

On opening the abdominal cavity, the visceral and parietal pelvic peritoneum was found inflamed and bathed in a dirty sero-purulent fluid, an evidence of fibrino-purulent peritonitis. Near the bladder the peritoneum looked dark and very ugly, and in a few hours, without irrigation and drainage, would have been attended by diffuse suppuration or septic peritonitis. The cavity was irrigated for some time with hot sterilized water. The lower end of the peritoneal wound was closed with a continuous silk suture; the cavity well drained with iodoform gauze, and silk-wôrm sutures placed in such a position as to close the abdominal wound after removal of the gauze. The patient was put to bed in a fairly good condition; pulse 120, temperature 101.2°. He rallied well, with a steady decrease of temperature, until May 26, at 2.30 P. M. The drainge had ceased, the gauze was removed, and the abdominal wound closed; temperature 100.6°, pulse 88, respiration 26. By 7.30 P. M., temperature 102°, pulse 89, respiration 26. A dose of salts was given, and at 9 P. M. an enema was administered with good effect, reducing the temperature to 100.8° by 10 P. M.

As he had been much nauseated, the patient was fed and stimulated by enemas. The bladder commenced to drain through the suprapubic wound on the sixth day after operation, showing that the sutures in the bladder had given way. The catheter was removed from the urethra. The abdominal wound soon became infected and sloughed somewhat. The abdominal stitches were removed on June 2d, and the wound dressed with chloral solution. From this time the temperature varied from 98.8° to 99.6° until June 17, when at 6 A. M. the condition was: temperature 101°, pulse 102, respiration 24; at 5 P. M., temperature 103.6°, pulse 116, respiration 26; delirious and in a condition of profound sepsis. The origin was difficult of location. When not delirious he would complain of great pain in the head, back and abdomen, and of nausea. Refusing all nourishment, he was fed by nutritive enemas, and also was given one grain of calomel every hour until six grains had been taken; then a full, high enema was given which proved very effective.

June 18, 9.15 A. M., temperature 100°, pulse 105, respiration 24; complained of feeling chilly, still nauseated and suffering pain in his head and abdomen; 5 P. M., temperature 103°, pulse 100, respiration 26. This condition continued, with morning temperature about 100° and afternoon temperature 103°, until June 21 at 3.30 P. M., when he became very restless and weak, suffered intense pain in his back in the region of the right kidney, aching in the limbs, head and abdomen; pulse 120, temperature 104°, respiration 34. Sponge baths of iced water and alcohol were ordered to be given every two hours, and the nurse was told to push stimulants and nourishment. The next morning at 8.30 a great deal of pus escaped from the suprapubic wound and continued for several days. Whenever an enema was given to wash out the bowel, he would pass urine through the urethra, which would be filled with pus. Temperature remained up until June 23, 4 A. M., when his condition improved. He looked and felt better; temperature 100°, pulse 84, respiration 22. June 25, he again suffered from absorption of pus, temperature going up to 103.4°. The wound was washed out, as had been done before, with peroxide of hydrogen, every six hours. By the 28th the temperature was normal, but would vary during each day, going as high as 100°.

The poor little fellow's troubles had not yet ceased, for on July 7 he had another rise of temperature, it being at 6 P. M. 103.8°, with all symptoms of sepsis. The wound was drained well and freely, so another focus of infection had to be sought. Later in the evening he passed a large quantity of decomposed mucus from the rectum, and continued to do so three or four times daily for several days, the temperature varying during this time from 101.5° to 103.8°. The trouble in the rectum was due, no doubt, to the effect of the nutrient enemata he had been having so continuously during his illness. After washing out the bowel for several days the temperature was again brought down to normal by the 10th, remaining so until July 14, when it again rose to 102.6°, from the same cause in the bowel. After washing out the latter for a few days it fell to normal and remained so. To-day he was removed to his home in this city with the abdominal wound nearly healed, water being passed *per via naturalis*, and in an apparently good condition after nine weeks' illness.

Several interesting points might be brought out in reviewing this, but, in concluding, I will only refer to two: 1. Suppurative septic peritonitis may follow suprapubic cystotomy without mechanical injury to the peritoneum. 2. The early, prompt and thorough flushing of the abdominal cavity, followed by drainage for suppurative, septic, peritonitis, will save life, where a few hours' delay would only bring surgery into disrepute, and the patient's life would be doomed.

SELECTIONS.

THE CLINICAL VALUE OF MICROSCOPICAL EXAMINATIONS OF SPECIMENS REMOVED BY THE CURETTE.*

By HENRY C. COE, M.D., New York.

This is a subject which is naturally of great practical interest to us, because every surgeon has cases submitted to him in which it is extremely important to arrive at an exact diagnosis with regard to the condition of the corporeal endometrium. Now, in the very cases in which we are most in need of help from the pathologist we unfortunately cannot always receive it, for various reasons. The surgeon stands necessarily in a different attitude toward the patient from the pathologist. The latter is called upon simply to give a more or less definite opinion based upon the microscopical appearance of a certain specimen. The surgeon, on the other hand, is expected to give a positive opinion to the patient or to her friends, an opinion on which may depend not only her future peace of mind, but also the decision with regard to a radical operation; and sometimes he is placed in a painful position of uncertainty. The object of this discussion, I infer, is to elicit the results of our experience with regard to the amount of information which is furnished by the microscope in doubtful cases.

There is considerable literature on the subject. If you will glance at any of the recent editions of the text-books you will find that the microscopical test is generally regarded as the crucial one. After dwelling upon the symptoms of malignant disease of the corpus uteri, the writer invariably concludes by saying that the final test is the examination of curettings. Now, this would lead the inexperienced reader to believe that the microscope will always settle the question definitely, but unfortunately it does not. A paper by Dr. Hunter Robb in the last volume of Gynæcological Transactions contains reports of one hundred cases in which he examined curettings from the uterus. He is known to be a careful man, and his conclusions should be received with due deference. Out of these one hundred cases he states that in ninety-five he found that he was able to make a positive diagnosis by the microscope. That is certainly a much larger percentage than most other men can show, but he goes on to qualify this by saying that in sixty-three cases in which the symptoms indicated a benign condition the microscope showed the absence of malignancy, while in only two cases in which malignant disease was diagnosed did the microscope confirm the diagnosis. In twenty-six cases in which malignant disease was suspected its absence was shown by the examination of the tissue removed. In other words, his results show that the negative evidence furnished by the pathologist in these cases is more valuable than the affirmative.

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* Discussed before the New York Obstetrical Society, March 16, 1897.

I would like to refer briefly to two or three illustrative cases. One patient had been under the care of a physician for two years. He curetted her uterus two or three times, examined the scrapings very carefully (being a competent pathologist), and made a diagnosis of adenoma. On account of persistent hæmorrhages, without pain, he sent her to me. I decided that it must be a case of malignant disease; received her into the Cancer Hospital and she was examined under ether. I curetted and removed a quantity of brain-like material. It was examined under the microscope and pronounced positively to be simple adenoma; nevertheless, vaginal hysterectomy was performed. Sections were made of the uterus, they were repeatedly examined, and the pathologist pronounced it non-malignant. The patient has now, after a year and a half, an extensive recurrence. Another case was that of a young woman who had atypical hæmorrhages, not only menorrhagia, but metrorrhagia, without pain, her health being good. I curetted and obtained a quantity of very suspicious material, which was examined and pronounced to be ordinary fungosities. I promptly removed the uterus. Sections through the organ showed undoubted carcinoma.

Another recent case called my attention still more forcibly to the difficulty of making an exact diagnosis. The patient had been under the observation of a physician in a neighboring city for nearly a year. She had had, within the last six or eight months, profuse menorrhagia, and during the last three months she had had metorrhagia more or less constantly; there was no foul discharge, and her general health was fair. She had been having for the last three months shooting pains in the back and abdomen. I examined, with a colleague, her under an anæsthetic, and found the uterus enlarged. I introduced a curette and removed a drachm of soft, brain-like material. I said at the time that this was probably a case of malignant adenoma of the body of the uterus, and rather than have the woman return home unrelieved, I thought that hysterectomy was certainly indicated. Two pathologists pronounced that there was absolutely no sign of malignancy, the condition being simple adenoma. I examined some of the slides, and could myself find no evidence of malignant disease. I stated the case clearly to the patient and to her husband, and after a good deal of vacillation, they finally decided to have a radical operation. He reconsidered it several times, and when she was on the table asked whether I could swear that it was malignant, as he would not have the operation unless I could. I advised him to take his wife home at once, which he did. I have not the slightest doubt that the woman will return with well-marked malignant disease. (She did return three months later, and, on the advice of the late Dr. Lusk, I performed total extirpation successfully. The diagnosis was fully confirmed.)

I have mentioned these cases simply to show the doubtful position in which we often stand. Another case was that of a patient who was passing through the menopause. Although her general health was fair, she had had pelvic pains for a long time; she had irregular hæmorrhages, not only menorrhagia, but metrorrhagia; and had a peculiar bloody and rather offensive discharge. I placed her under ether twice, examined and curetted her thoroughly, removing a quantity of suspicious tissue. This was examined microscopically, and in both instances was pronounced to be hyperplastic endometritis. I confidently expected that a radical operation would eventually be required, but she has had no return of the hæmorrhage. Over a year has elapsed, and I have no reason to believe that there was any malignant element in that case, and yet she had a large soft uterus which bled easily, and the tissue removed presented all the macroscopical appearances of malignant disease.

I need not go into the clinical history or lay stress on the one symptom which induces us to resort to the curette for diagnostic purposes, that is, hæmorrhage. If we are to accomplish positive results we must get hold of these cases in their incipient stage, consequently we should always look with suspicion on atypical uterine hæmorrhage. We know that ordinary menorrhagia is generally a symptom of hyperplastic endometritis, or of decidual remains, and we also know that atypical hæmorrhages after the menopause are extremely important; but we must remember that even after the menopause we may have such hæmorrhages due not to cancer, but to senile erosion, or endometritis. Those who are accustomed to curette judge a good deal by the condition of the uterus at the time of the operation, and by material removed. The macroscopical appearance of the hypertrophied mucosa is so characteristic as compared with the soft, brain-like material of adenocarcinoma or sarcoma, that we feel pretty certain from the gross appearance, in connection with the clinical history, what the condition is with which we have to deal. I need not allude to the appearance of retained decidua, or of the pseudo-decidua of ectopic gestation. A description of the histology belongs rather to the pathologist, but the practical surgeon is sometimes very much puzzled by the pathological report; he can not understand exactly what is the criterion of malignant disease.

The claim is made by the pathologist that he often fails to receive a typical specimen; that is often true, but we give him the best that we can. The sharp curette, even when vigorously used, may fail to remove the desired tissue. Usually, by proper curettement covering the entire surface we ought to obtain some good specimens, but we can hardly be expected to dig out a portion of the uterine wall. I think that a mistake is sometimes made in demanding too much of the pathologist. We submit with the specimen a scanty history, or none at all, and expect him, from an inspection of the microscopical sections, to deduce the entire history and prognosis of the case, very much as a paleontologist would reconstruct a fossil animal from a single tooth. We should send him a clear history of the case, and mention the suspicions which we entertain, and allow him to support or to disprove the diagnosis as he can.

Now, the practical question comes up, how much can we depend on the pathological report, and how much upon the clinical history? It seems to me that we might divide the cases into two classes. The first includes younger women, in the prime of life, with irregular hæmorrhages, where we suspect malignant disease. In these cases, unless the history is pretty strong, we should consider the case as doubtful until the fact of malignancy is proven. The microscope will help us in those cases rather by the negative than by the positive information which it affords. But, when we come to the other class of cases, including women of advanced years, with a fairly well-marked history, especially of obstinate atypical bleeding, when we remove material which has a suspicious appearance, we are justified in making a diagnosis of malignancy even though the microscope throws no positive light upon it. I would not be understood as advising any man to go ahead recklessly and remove the uterus without carefully weighing all the evidence, but the question arises: Shall we ask a patient in this condition to wait a year or two until she has passed the stage at which the operation will be curative, because we are not entirely certain in our own minds? It seems to me that it would be better for us to occasionally remove a non-cancerous uterus, rather than to run the risk of allowing the patient to go on until the disease becomes inoperable. It has sometimes seemed to me, with all due respect to our friend, the pathologist, that pathology has not made as many advances in the last ten years in this particular direction as in some others; in other words, that we are not as near to making a positive diagnosis by the examination of curettings as we might be. This is not offered in criticism of the pathologist, because we can not expect him to accomplish impossibilities, and I do not think any pathologist should make a diagnosis of malignant disease to accommodate the surgeon, or in order to fit in with the history of the case; but I regret that we have not some more positive microscopical test of malignancy in these doubtful cases.—The American Gynæcological and Obstetrical Journal.

THE DIFFERENTIAL DIAGNOSIS BETWEEN DISEASES OF THE SOUND CONDUCTING AND SOUND PERCEIVING APPARATUS.

By EDWARD B. DENCH, M.D., New York.

While the physical examination of the organ of hearing has been practised since the earliest days of otology, the functional examination seems to have been neglected until a comparatively recent period. While I do not wish to, in any way, underestimate the value of otoscopy, I think all will agree that, in chronic cases, the information obtained by ocular inspection is often entirely insufficient to enable the observer to determine the degree of impairment of function. We not infrequently find evidences of either a slowly progressive inflammatory process within the middle ear, or the results of a previous acute inflammation. The otoscopic picture may differ widely from the normal standard, yet, the hearing may be scarcely affected, and any impairment which exists may cause the patient no inconvenience. Careful tests will, of course, show in these cases, diminished audition for certain sounds; for instance, the watch may be heard less acutely than by the normal ear. The voice, however, may be well heard, notwithstanding the changes which have taken place in the conducting mechanism. On the other hand, an examination with the otoscope may reveal no deviation from the normal standard. The drum-membrane appears natural in color, position, luster, and structure, and yet, the hearing may be greatly impaired. Thanks to the labors of Bezold, Schwabach, Rinne, Wolffe, and others, the otologist no longer relies upon the information obtained by physical examination.

For convenience in the location of the pathological processes met with in the organ of hearing, the ear may be divided into two parts, first, the conducting mechanism, and second, the perceptive mechanism. The conducting mechanism comprises the auricle, external auditory meatus, drum-membrane, and the tympanic cavity and its contents. The perceptive apparatus includes those parts lying beyond the foot-plate of the stapes, namely, the bony and membranous labyrinth, the terminal filaments of the auditory nerve, the nerve trunk itself, the basal nuclei, the cortical auditory areas, and those collections of nerve fibers which join these nuclei to one another and afford communication between them and other cerebral ganglia. It is well known that any affection of the conducting mechanism is upon functional examination characterized by certain definite signs. This is true without reference to the precise situation of the pathological process. In other words, obstruction to or interference with sound conduction always produce the same phenomena. These signs may be slightly modified, according to the location of the lesion, but remain essentially constant. On the other hand, diseases of the perceptive mechanism give rise to a series of phenomena which differ entirely from those produced by diseases of the conducting apparatus.

In order to locate any pathological process in either the conducting or perceptive portion of the organ of hearing it is necessary, in the first place, to know what may be termed both the quantitative and qualitative audition of the normal ear. It is a well-known physical law, that the intensity of sound varies inversely as the square of the distance from the sounding body, hence, the quantitative audition is easily determined by comparing the distance at which any sounding body is heard by the normal ear, with the distance at which it is perceived by the patient. Many attempts have been made to devise an instrument which would produce a sound, the intensity of which would be invariable. The ideal instrument has not yet been found, and the quantitative test is ordinarily made either with the watch or with the acoumeter of Politzer, the percussion note of this instrument being fairly uniform.

In order to determine the qualitative audition, the ear is tested by a series of musical tones. The normal ear perceives as a musical note the tone emitted by a sounding body, the vibrations of which are repeated at regular intervals sixteen times each second. This is the lowest note recognizable by the normal ear and is called the lower tone limit. Under normal conditions, increased rapidity of vibration of the sounding body is recognized by an elevation in pitch of the note, until the vibrations are repeated more than 32,500 times each second. Beyond this limit, the sound is no longer heard, the rate of vibration being too rapid to make any impression upon the organ of hearing. The note produced by 32,500 vibrations each second may, therefore, be called the upper limit of audition, or the upper tone limit.

A careful examination of a series of cases shows that when the conducting mechanism is affected, not only is quantitative audition impaired, but also that qualitative audition is affected in the following manner: the ability to perceive the lowest tones of the musical scale is interfered with in direct proportion as the quantitative impairment increases. In other words, the lower tone limit is elevated. This is true, no matter where the obstruction to sound conduction exists. The interference may be caused simply by the occlusion of the auditory canal by a mass of cerumen, or it may be due to increased tension in the ossicular chain. In all cases, low tones will be poorly heard, and the lowest notes may not be perceived at all. On the other hand, if the disease lies in the perceptive mechanism, the lower tone limit may perfectly normal, although quantitative audition may be greatly diminished. When we observe the effect of disease of the conducting and perceptive mechanism upon the upper tone limit, we find a similar reversal. In diseases of the conducting mechanism, the high tones are heard exceedingly well, the upper tone limit being, in some cases, above the normal standard. Where the nervous portion of the organ is involved, the reverse is true, the upper tone limit being considerablv reduced. This is particularly the case, where the affection of the perceptive apparatus is secondary to an inflammatory process within the middle ear.

The lowering of the upper tone limit, in these cases of secondary labyrinthine involvement, is easily explained, as the portion of the labyrinth immediately adjacent to the middle ear is concerned in the perception of the highest notes of the musical scale. In primary labyrinthine disease, however, a considerable lowering of the upper tone limit is usually found. The reason for this has always been largely a matter of conjecture, but it has seemed to me that it might be explained by the fact that the lower part of the labyrinth being more freely supplied with blood vessels than the upper portion, is consequently, more susceptible to the circulatory changes. Any obstruction to the venous return current would, therefore, be more pronounced in this portion of the cochlea than near the apex. The presence of tone gaps is so classical a symptom, that it needs simply to be mentioned. It should be borne in mind, however, that complete deafness for certain tones is not an absolute indication of an organic lesion of the labyrinth.

In addition to the qualitative tests, much information is gained by the determination of bone conduction. As we all know, in diseases of the conducting apparatus, the bone conduction is exaggerated, the reverse being true when the perceptive mechanism is affected. Certain facts must be kept in mind, however, in determining bone conduction, in order to avoid erroneous conclusions. I am aware that certain investigators have declared that the conduction through the solid media of the skull, is practically unaffected by the age of the patient. My own experience, which has been rather large, convinces me that such is not the case. In almost all patients over 40 years of age, in whom the hearing is perfectly normal, the vibrating tuning-fork placed upon the mastoid will be heard for a considerable shorter period than in younger patients. After the age of 50, unless there is some obstruction to sound conduction, the tuning-fork placed upon the forehead, in the median line, may not be heard at all, although it is perceived fairly well over the mastoid process. A slight diminution, therefore, in bone conduction should not be considered as absolute evidence of organic disease of the perceptive mechanism in patients who have passed the fourth decade.

Some care is necessary in choosing a proper fork for the determination of bone conduction. The one I usually employ makes 256 double vibrations per second, and is, I think, best adapted to the large majority of cases. If a fork of lower pitch is chosen, the vibrations are apt to be felt rather than heard, while, if the pitch is more than one octave higher than this, the perception of the note by air conduction is apt to interfere, unless the patient is very hard of hearing.

In cases where a certain amount of labyrinthine involvement has followed a chronic non-suppurative or suppurative otitis media, it is often of value to determine exactly how much of the impairment of function is due to middle ear inflammation, and how much depends upon labyrinthine disease. This can be determined in a fairly accurate manner by a comparison of the air and bone conduction, for various notes of the musical scale. It will be found that, where the defective audition depends principally upon the middle ear lesion, the point in the musical scale at which air conduction becomes greater than bone conduction, will be directly in proportion to the degree of auditory impairment for the whispered voice, that is, if the whisper is very poorly perceived, a tuning-fork making 512 vibrations per second will be heard when placed upon the mastoid, after it is no longer heard in front of the ear. With an extreme amount of impairment of hearing, dependent upon labyrinthine changes, air conduction will be greater than bone conduction, excepting for the very lowest notes. I have found this means of diagnosis extremely valuable in determining the advisability of middle ear operations in these cases. Perhaps a single case, taken from my records, may illustrate this point more clearly:

A gentleman about 45 years of age, consulted me for the relief of a distressing tinnitus, and marked impairment of hearing in the left ear. Speculum examination showed a previous inflammatory process within the middle ear. The drum-membrane in the upper and posterior portion was quite thin, and the incudo-stapedial articulation could be easily seen through the membrane. The appearance was strongly suggestive of the development of adhesions about the stapes, as the result of the previous middle ear disease. In addition to the tinnitus and impairment of hearing, the patient suffered quite frequently from attacks of vertigo. He had been treated by a prominent otologist by means of catheter inflation, without relief; in fact, according to his own statement, the condition was growing steadily worse. Upon functional examination, I found that the lower tone limit was 64, although the whisper could be heard only at about 12 inches from the ear. The Eustachian tube was perfectly patent, and the air entered the tympanum without the least difficulty. Inflation failed to improve the hearing. I decided at once that the middle ear affection had run its course, so to speak, and that the patient was suffering from an increased pressure upon the labyrinth, due to the previous middle ear inflammation. From the history of the case, the indication seemed to be to relieve labyrinthine tension, without reference to the previous condition of the middle ear. The man was put upon pilocarpine, in increasing doses, until the constitutional effects of the drug were obtained. Under this plan of treatment the whisper increased to 12 feet, and the watch, which could not be heard at all when I first saw him, could be perceived at 18 inches from the ear, the vertigo disappeared, and the tinnitus was slightly diminished, although less relief was experienced from this symptom, than from the others. The subsequent history of the case shows that the effects of the treatment have been permanent. Here, the physical appearance of the parts was so strongly indicative of a middle ear lesion, as to warrant the institution of measures for the relief of this condition, and it was only upon functional examination that the true nature of the lesion was discovered.

It may not be out of place, in this connection, to mention certain precautions necessary in conducting a functional examination. When we come to deal with tuning-forks of a very low pitch, that is, with those making less than 64 vibrations per second, it is often difficult to obtain the fundamental tone of the fork, on account of the presence of overtones. If the fundamental tone is not obtained, the patient will, naturally, hear the overtones, and an error will be made in determining the lower tone limit. For this reason, the surgeon should always hold the fork before his own ear before making the test, in order to ascertain that he has secured a perfectly true tone. If overtones are present, they may be "cut off" by applying the fingers to the limbs of the fork, close to their junction. In the determination of the upper tone limit, it is always well to give the patient no suggestions as to the sort of sound which he is expected to hear. As the Galton whistle is the instrument ordinarily employed, it is well to have the patient close the eyes, and then to begin the test sufficiently low in the scale to obtain a clear and distinct whistle. The tube is then shortened to almost the zero mark, the bulb compressed, and the patient asked to describe the sounds. He will probably say that the first was a whistle, the second, a puff. It is now very easy by increasing the length of the tube to discover the point at which the sound ceases to be a puff, and becomes a musical note. If this is not done, the result is often erroneous.

I hope that I may be pardoned for bringing such an elementary subject to the notice of this distinguished Society. It has often been my experience, however, that, while difficult and obscure subjects are zealously studied and mastered, elementary principles are often neglected, and serious errors are the result.

THE PASSING OF THE REFLEX — AN ADDRESS ON THE REFLEX ORIGIN OF NERVOUS DISEASES.

BY CHARLES L. DANA, A.M., M.D.

You must be a trifle tired at night of hearing the stories of cases, and records of symptoms, and points of diagnosis. So I shall beg to take you away a little from the beaten track of your daily clinical studies, and engage your attention in rather more abstract themes, not that the subject of reflex diseases is itself a theoretical one; but I want to discuss its history, its theory and its significance, rather than illustrate it with recitals of my experience.

And you will pardon me if in my discourse I drop into some unhackneyed phrases, or wander away from the narrow and austere course which is proper for a strictly scientific paper. Horace tells us that it is sometimes advantageous desipere in loco, and I perhaps shall not be to-night entirely conventional and self-contained in my discourse.

Hope and human sympathy are the exhilarating and helpful elements of life. They give charm to social intercourse, and specifically they ornament and make more effective our art. But, let me add, they ever tend to injure and weaken our science. And the doctor is always in danger from their enticing spell. With their help he cheers his patients, succors his own despondency, and is stimulated to new and ingenious efforts; but they distort his judgment, befog his logic and continually swerve him from the path towards the eternal verities which he should forever tread. Hope of curing and sympathy for the unfortunate lead to a continual desire to explain something, to unravel mystery, to detect causes at any cost, and to do something objective and tangible for disease. When they breed in the enthusiastic mind of persons with a highly technical training and bias, they lead to marvelous exploits in surgery and therapeutics, and to ingenious and fantastic theories of disease. I do not say all this in order to ask you to abandon these beautiful emotions, but only to indulge in them wisely.

The conception of the reflexes was not the result of any sympathetic sentiment or instinctive feeling. It was the legitimate outcome of careful scientific research; but the application of the dogma to-day to practical medicine and surgery has become in a great measure the expression of a dangerous misconception and misapplied sentiment. The doctor finds himself confronted with a disease which is obstinate and painful. It resists the ordinary therapeutics and the natural remedial power of nature. He feels, however, that he must do something. With the kindest feelings in the world he invokes the reflex arc, and if the pain is in the back, attacks the uterus; if in the head, cuts the eye muscles; if it is in the lungs, burns out the nose; if in the mind, he treats the liver, and so in a hundred ways he assails the unknown by annihilating some particular and reachable viscus. I am not admitting that any baser feeling ever enters. Ambition, vanity, the desire to exploit oneself, auri sacra fames, the greed of gold, are everywhere, but I do not count it as a factor here.

I do not believe that medical men generally realize how entirely our pathology is bound up and interwoven with conceptions of the reflexes. Three thousand years ago Hippocrates suggested that hysteria was a disease which was dependent upon disorder of the womb, and at the present day the physician cannot rid himself of the idea that the hysterical woman must necessarily have some disorder of that organ; yet facts show that true hysteria occurs in men and in children of both sexes, and there is plenty of evidence to show that women with their pelvic cavities nearly emptied can still have a typical hysterical attack. Marshall Hall, who was the great high priest of the reflexes in the early part of this century, stated that the principal causes of eccentric epilepsy were only three: "The presence of indigestible food in the stomach, the presence of morbid matters in the intestine, and uterine irritation." The profession is still profoundly under the influence of such statements.

The theory of reflex action was first distinctly recognized by Des Cartes, but it was first brought into medicine by Dr. Robert Whytt over one hundred years ago. The physiology of reflex action, and the importance of it in the phenomena of the body, were most diligently worked out in the years succeeding Whytt, by Prochaska in the last century, and by Mayo, Marshall Hall, Muller, and Ludwig in this century. It is now almost half a century since the facts regarding the importance of the reflex loop were given to science. Since then, the application of these has been given to the practical art of medicine, with the ever-widening circles of the alluring but delusive therapeutics. When the history of medicine of this century comes to be written, the reflex loop will be found to be filled with the dangling remains of unsuccessful therapeutical and surgical endeavors, like the ropes on wash day in the rear of a New York tenement.

There was once a distinguished surgeon, Dr. Robert Battey, who conceived the theory that the normal ovaries were the starting point of a large array of chronic ailments, and he reported some fourteen cases of epilepsy cured by removing the normal organ. Not more than two decades ago, it was believed that paraplegia could be caused by reflex irritations from the intestines, the bladder, and other organs. Literature of the last thirty years has been filled with the histories of reflex neuroses from the ear, from the eye, from the teeth, the nose, the stomach, and every organ which could be reached by the instrument of the surgeon or the manipulation of the physician. We read of reflex amblyopia, reflex deafness, or reflex epilepsy, paraplegia, chorea, tetanus, aphasia, neuralgia, and headache.

There is already some reaction, but I have a feeling that there is yet much misconception of the matter.

It is my desire to-night to present to you what I conceive to be the true position the physician should take in regard to the importance of these reflex impulses, and you will see that while I do not ignore their value I want especially to impress upon you the importance of attacking in your therapeutics the centers of the disorders rather than of nibbling away at its outer edge. If you are going to make a fruitful and luxuriant growth, you need much more to water and till about the roots than to prune and graft the branches. This is the purpose which I hope to make you feel underlies my discourse.

My first and perhaps my chief general proposition is this, that there are no reflex constitutional neuroses. By this I mean that those neuroses which are general in character, and which are classed by systematic writers as constitutional, and are not dependent upon reflex irritants, and cannot be cured by attacking these. Such neuroses are, in particular, chorea, the functional spasms, such as convulsive tic, epilepsy, paralysis agitans, even hysteria of the true, the major type as I have described it in my lectures here and in my text book, is never dependent upon a remote local irritation. Spinal irritation, in its typical form, and the neuralgias of a constitutional or degenerative type, such, in particular, as migraine, in early life and the tic douloureux of middle life, may be added to the list.

Let me now take up some of these in turn, and give you more specifically the evidence in support of my statements.

Chorea.—There are very few physicians who still maintain that chorea is a reflex neurosis. A certain limited number, however, do assert that by proper treatment of the eye muscles, the chorea can be cured, and particularly, those forms of chorea which are more properly classed with the convulsive tics, or which are often known as "habit" and "chronic" choreas. So far as the spasmodic movements are concerned, all that eye and nasal irritation can cause is a certain degree of twitching, that is to say, those muscles which are intimately associated with the movements of the eye may be reflexly kept irritated and in a state of mobile spasm by eye disease.

Epilepsy.—As for epilepsy, it will, I think, be a great relief to some, and a great shock to the traditions of others, to be assured that true epilepsy is always a central disease. It is not caused essentially by reflex irritation, and it is not cured by treatment of the eyes, or nose, or teeth, or stomach, or bowels. It is not cured by taking out the ovaries, or by the removal of the uterus, or readjustment of the kidneys, nor even by phimosis, or by removal of the appendix. You will find in literature a good many cases in which epilepsy is alleged to have been caused by some scar or bite, by the presence of worms in the intestine, or by a blow upon some part of the body, and there are cases in which cures have been reported from cutting off the scar, removing the worms or the offending body or organ. It is very interesting and instructive, however, to note that these cases increase in number the further we go back in the literature of medicine, and that with each succeeding year, their number in current literature diminishes. For my own part, in a list of epileptic cases, which numbers over three hundred, I have never come across any such instance. Dr. Emmet cited a case of epilepsy cured by the excision of a cicatrix in the cervex uteri. Dr. Weir Mitchell tells me that he can recall three cases of epilepsy in the total range of his life experience apparently cured by removal of a local irritant. He tells me, however, that he thinks the views I here lay down are essentially correct.

It will, I think, be found that nearly all cases of remarkable cures from removing some local trouble are either temporary, or that the central trouble is not true epilepsy, or has not fully established itself as such. Let me be distinctly understood, however, upon this point. Given a person of epileptic or convulsive tendency, a neuropathic individual, then by some blow or injury or eccentric irritation, I grant that a convulsion may be produced. This convulsion may be repeated for a few times, and then, if the cause is removed, it may subside. But in this instance the individual has not yet developed true epilepsy. He has simply suffered from some symptomatic convulsions. In the vast majority of cases, however, even if the cause is removed, the injury has been done, the disease has begun, and its cure is only effected if at all, by a general treatment. In epileptic persons the nervous balance is very delicate. The instability of the brain cells is such that a very slight cause will tip them over, so to speak, and a convulsive discharge will result. Now, when this exists, it is imperative that all reflex excitants be removed as much as possible, and if this is done, the number of attacks will be lessened, but they will not be stopped. We can, therefore, often moderate the severity of epilepsy by treating the eyes, the stomach, the pelvic organs, and by doing everything else which will lessen the eccentric irritation of the nervous centers. But to cure the case we must find something that will make the brain cells more vigorous, and will stay the progressive tendency to degeneration of the brain cells. They are endowed with feeble vitality, they are over-irritable, they constantly break down and explode, and it is these invalid cells that must be nourished, looked after; not the eyes, or the nose, or the teeth, or the stomach.

As to the relation of epilepsy and eye-strain, it may be known to you that several years ago a committee of the New York Neurological Society examined into this subject, their investigations extending over two years. They reported that they could find no evidence that treatment of muscular or refractive defects of the eye caused either epilepsy or chorea.

Noyes, who viewed this subject with a liberal mind, says in his text-book, that "in a few cases eye-strain may have been an exciting cause, but there has been behind it a deeper lesion of the nervous system.

Every year, says Gray, there is a case of epilepsy reported cured by the removal of a tooth. But, he adds, no one believes that the fons et ongo mali is a peripheral irritation.

Lusk asserts that the performance of normal ovariotomy for epilepsy is to be regarded as "hardly better than malpractice."

Neurasthenia.—There have been periods in the history of this disorder when all neurasthenia was thought to be due either to eye-strain or uric acid. There was a time also when hypochondriasis was thought to be a matter of constipation and the liver. It is not worth while taking up your time now in refuting these views. For while it is acknowledged that they may all be factors in breeding nervous symptoms, there is something behind them that is more important, and that is a naturally defective or an exhausted nerve center. I have seen neurasthenics vastly improved for months by tenotomy of the eye muscles, and by washing out the stomach, and careful attention to the diet, but when the condition is anywise a fixed one, these measures alone are ineffective. A great deal of help can be given sometimes to neurotic and neuralgic women by scraping the uterus, sewing up tears, and replacing the dislocated organ. But often it is the rest in bed and relief from care, as much as the operation, which helps when help is really given.

As for the results of more serious surgery, Playfair cites a number of cases in point, and asserts that the removal of the appendages "is not a legitimate procedure in cases of purely functional neuroses."

Spinal Irritation.—Women with pain in the back are often relieved by treatment of the pelvic organs. There is a disease, however, known as spinal irritation, in which the whole back is painful and sensitive; the patients are nervous, neuralgic, have headaches, are easily exhausted, and show signs of hysteria and neurasthenia. Here the trouble is constitutional in part and local in part, but it is not uterine, or ovarian, or gastric, or renal reflex. These patients are helped by sthenic measures, gymnastics, braces and counter-irritation. They need not have their kidneys pinned up or their appendix resected, the clitoris freed from adhesions, or the uterus and its ovarian allies removed.

Insanity.—It would be going too far for me to discuss the question of the reflex origin of insanities. No doubt reflex irritation is one factor in some puerperal and menstrual psychoses. But heredity tendencies, auto-toxæmia and other agencies are much more powerful. Personally, I do not meet insanities which are due to reflex irritation, nor do I find any case cured by local treatment, though my friend, Dr. Wylie, states that he has had such.

I admire immensely the surgical skill and daring of the present day. Its effect, on the whole, has been enormously beneficial. But I would remove from its aggressive enthusiasm the reflex arc. This arc is a physiological and anatomical fact of too much intrinsic beauty and vital importance; but we have made of it a rainbow of seductive and unwarrantable promise, with an occasional pot of gold at one end. The new woman is especially imperiled. She used to have to watch the hymen; now she must look out for the appendix, the adnexæ and the mobility of her kidney with as much care as if they were her cardinal virtues.

As I admire the surgeon, the gynæcologist, the neurologist, and the specialist in general, so I would say a word for the reflex, as well as against it. The reflex is an excellent thing, and not in the least to be ignored. It wards and warns us against danger, it is the safety valve of our automatic life, it regulates the secretions, it steadies our circulation, and it is ever building itself up into combinations that protect and conserve the higher machinery of life. But in the evolution of man the part played by the reflex becomes relatively smaller and smaller. The amoeba is all a reflex; man is mostly made of automatic and conscious life. The activity of the lower animal is constantly and almost wholly guided by the changing stimuli that impinge upon his body, and arise in his viscera. But man's life and actions are most influenced by causes that are centric, by the stored up impressions in the brain. The higher in evolution, the less important the reflex in essentially modifying life. Compare the daily movements of a rational human being and that of a dog, and see if I am not right. If you take a frog and strike a smart blow on its epigastrium, you can cause the heart to stop. This is a reflex act, which does not occur except in a slight degree in higher vertebrates. Levisson exposed the kidneys in a sheep and found that by severe compression of them he produced paraplegia; he brought about the same result by squeezing the uterus. Veterinarians report cases of paraplegia following injury of the testicle, and many physicians must be familiar with the posterior paraplegia that occurs in horses after severe wetting and exposure. These cases may be and probably are reflex and functional conditions. But they do not occur in man. He has risen a step higher and his inferior extremities are no longer easy slaves to his cutaneous and visual impressions. The mind-stuff is the important thing in man, the medulla-stuff in the animal. The new man and the new woman are evolving away above the reflex loop. It is the only hope of the angleworm that this be preserved, but it is a reaction to lower types for man to be subject to it. Do not hope, therefore, to cure disease by removing the irritant from a sensory end organ. Remove it, of course, but do not believe that you are then done.

Gentlemen, I have, perhaps, spoken too lightly of the importance of peripheral irritation, of a loaded stomach, an astigmatic eye, a displaced uterus. Pray do not misunderstand me; I wish only to make you feel that the man is greater than his parts; that to study his general condition is better than to squint into any particular viscus. I have seen migraine restored by glasses, vertigo cured by removed wax from the ear, intense spinal pains relieved by replacing the uterus. I have seen wretched neurasthenic women made over by gynæcological operations, and I render in full my sense of the obligation of medicine to its special workers. But I repeat again that fixed diseases are never cured by treatment of reflex irritation. There are reflex pains, reflex spasms, reflex mental disturbances; there are reflex symptoms, many and important, but there are no reflex diseases in any true and serious sense of the word. If I have succeeded in impressing this upon you, I believe you will be better physicians and more learned and clear-sighted men.

Gentlemen, hope and tenderness and sympathy, are beautiful things. They inspire and make glad our lives, and they stimulate us to try to cure our cases when we should know that we can't do it. Thackeray once said: "Fun is good, truth is better still, but love is best of all." For us as men, perhaps, but as practititioners of medicine, truth is best of all, and we should try to accept it and be guided by it.—The Post Graduate.

ABSTRACTS.

THE RAPID DIAGNOSIS OF THE LOEFFLER DIPHTHERIA BACILLUS.*

BY FRITZ SCHANZ, M.D.,
Dresden.

The conviction that in antitoxin we possess a specific remedy which protects the human organism against the injurious effects of Lœffler's diphtheria bacillus, must naturally give rise to the desire to recognize the affection caused by this micro-organism as quickly as possible before the clinical symptoms permit a conclusion as to its presence. Furthermore, the fact that the clinical picture of diphtheria is manifested by affections which have no connection with the Lœffler bacillus, and which, according to Fraenkel, we designate as "cocci diphtheria" in contradistinction to "bacillus diphtheria," must also urge us to as quickly as possible demonstrate the Lœffler bacillus in order to avoid unnecessarily subjecting patients to the treatment which is specific against the Lœffler bacillus alone. The latter point may now deserve especial attention since Barth[1] has shown that there can be no doubt that toxic action occasionally accompanies the administration of antitoxin.

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The need of recognizing the Lœffler bacillus as quickly as possible exists not only in the great institutions which have been provided with bacteriological laboratories for this purpose, but also among practicing physicians. In order to meet this need in practice, examination stations have been established in various localities, to which physicians can send the material obtained from patients suspected of diphtheria. This material is at once examined, and the physician informed of the result of the examination as speedily as possible. This method presupposes that the Lœffler bacillus can be diagnosed with certainty in at least twenty-four hours. At the present time, however, it is to be feared that this presupposition cannot be fulfilled. There can no longer be any doubt that there are bacteria wholly similar to the virulent diphtheria bacillus, which are widely distributed and have nothing to do with diphtheria. In a discussion at the Medical Society at Griefswald, Lœffler himself stated that very frequently there occurred in the oral cavity a saprophyte which it was difficult to distinguish from the avirulent diphtheria bacillus. From the short report, it cannot be determined whether by "avirulent" Lœffler means the "pseudo-diphtheria bacillus," which, according to the description given us by Hoffman and Lœffler, is distinguished from the genuine diphtheria bacillus only by the size of its colonies, and by the fact that it does not render alkaline bouillon acid in the same length of time as does the virulent form. Neither one of these points of distinction can be accepted at the present time; but even if they could be relied upon, the differentiation could certainly not be made in twenty-four hours. That this saprophyte must be very widely distributed is admitted by both Lœffler and Hoffman. For example, Hoffman found it 26 times in 45 oral cavities examined with this object in view; consequently, he designates it as a constant inhabitant of the oral cavity. That this declaration is true is shown by the statements of Roux and Versin, who designate the pseudo-diphtheria bacillus as avirulent Lœffler bacillus; they found it 15 times in 45 healthy children, and in 59 children from a sea coast village in which no diphtheria existed, 26 times. Bech found it 26 times in 66 children, 5 times in 17 cases of follicular angina, and 9 times in 14 cases of catarrhal angina. Judging from the uniformity of these results, there can scarcely be any doubt that the pseudo-diphtheria bacillus is one of the most frequent micro-organisms of the oral cavity, and that it can be found in the mouth of almost every second child. What value can be attached to a method with which, within the required time of twenty-four hours, it is impossible to distinguish the genuine diphtheria bacillus from a constant inhabitant of the oral cavity?

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[1] "Die Nebenwirkungen des Diphtherieheilserums," Deut. med. Woch.

The most extensive report upon the value of this method of examination is furnished us by Dräer[2], who publishes the results of 400 such tests made in the Hygienic Institute of Kœnigsberg. He states that an experienced operator can readily distinguish the pseudo-diphtheria bacillus from the genuine diphtheria bacillus. His opinion, however, is based upon but twelve animal experiments. He attaches but little importance to the test of virulence, because it is known that, under certain circumstances, even the genuine diphtheria bacillus may also undergo such a reduction in virulence as to render animal experimentation with them negative. Dräer, therefore, distinguishes the virulent diphtheria bacillus which has become non-virulent, from the pseudo-diphtheria bacillus. It would be of great service to us if he would state the points of difference by which he was enabled to distinguish them. Prof. C. Fraenkel[3] has shown that, aside from virulence, there is at present no essential difference between the pseudo-diphtheria bacillus and the genuine diphtheria bacillus, and that we, with the French, should designate the pseudo-diphtheria bacillus as avirulent diphtheria bacillus, especially as there is a possibility that the one can be transformed into the other. Recently Trumpp[4] succeeded in transforming non-virulent bacilli into virulent. Many observers, following the precedence of Martin[5], look upon the size of the bacilli as characteristic of their virulence. My researches upon the non-virulent Lœffler bacilli in the conjunctival sac have shown me that this feature cannot be relied upon; we find in the conjunctival sac very long bacilli possessing all the characteristics of the Lœffler bacillus, and these are non-virulent.

Recently, Spronck[6], of Utrecht, sought to find points of difference between the genuine Lœffler bacillus and the non-virulent bacillus in the conjunctival sac, which ophthalmologists formerly considered to be the etiological factor in xerosis. He admits the similarity of these micro-organisms in culture and preparations, and endeavored to find a new means of differentiating them, namely, by aid of the specific action of diphtheria antitoxin. He was led to this by his researches upon the bacillus which the French designate as the short, non-virulent Lœffler bacillus (bacille court). These bacilli were found to be weakly virulent—1 c.c. of a 24-hour bouillon culture produced edema in the neighborhood of the point of injection; 2-3 c.c. caused transitory general phenomena: loss of appetite, bristling of the hair, loss of body weight, relaxation. Guinea-pigs which were previously treated with relatively large doses of anti-diphtheritic serum were absolutely not refractory to this weakly virulent bacillus, while much smaller doses of the same serum sufficed to render innocuous the above-named quantities of a fully virulent culture of diphtheria bacilli. Spronck also tried this method of testing the xerosis bacillus, which he had cultivated from three cases of xerosis offered for this purpose. In this connection he refers to my publication[7], and reports that he was able to determine a constant but slight virulence in his bacillus, as I had done in one case—1 c.c. of a 24-hour bouillon culture, given to a medium sized guinea pig, caused edematous swelling, which was most pronounced about twenty-four hours after injection, and which usually disappeared in forty-eight hours by absorption; after subcutaneous injection of 2-3 c.c., this edematous swelling was much more intense, and slight general phenomena also appeared; loss of appetite, languor. The animals soon recovered and subsequently remained healthy.

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[2] Dert med. Woch., 1896, p. 279.

[3] Berl. klin. Woch , 1893, p. 252.

[4] Centralbl. f. Bact., XX, No. 20.

[5] Ann. d. l' Inst. Pasteur, 1893, p. 335.

[6] Deut. med. Woch., 1896, p 571.

For those who have made a special study of the xerosis bacillus, this constant reaction must be surprising. The researches upon the xerosis bacillus and its pathogenesis were carried out at a time when no one thought its similarity to the diphtheria bacillus would ever have any significance. At that time all workers came to the same conclusion, namely, that the bacillus was not pathogenic. However, at that time also, an animal now and then manifested a slight reaction; but this reaction was not constant, otherwise it would certainly not have escaped the attention of numerous investigators, and, when it occurred, it rapidly disappeared, so that it was not considered sufficient to stamp the bacillus as pathogenic.

After Fraenkel had found that by increasing the dose a reaction was also obtained from the mildly virulent bacilli, I also increased the dosage in the experiments mentioned, and secured the reaction which Spronck speaks of. At that time I conducted a further series of experiments with the same bacillus, with the object of increasing the virulence; but no such constant result was obtained as, in my opinion at least, would warrant its being considered a reliable criterion for such a test as Spronck mentions.

On the contrary, it appears to me that a new proof of the intimate relationship of both these species of bacteria can be brought forward. In the Hyg. Rundschau, Prof. C. Fraenkel reports a morphological peculiarity of the diphtheria bacillus. He found that in fresh cultures there sometimes occur forms which can only be interpreted as genuine ramifications. These forms occurred chiefly in growths on egg albumen, upon which a kind of "giant growth" showed itself. Here were observed branchings which appeared as minute buds, most commonly sprouting at right angles from the middle of the rod. On further cultivation, crutch-like forms appeared, and when the ramifications progressed still further, forms developed which remind one of a large H of Latin print. J. Bernheimer and C. Folger[8] report the same findings and also present a number of illustrations. All of these forms can also be found in the non-virulent bacilli of the conjunctival sac, as I was able to convince myself at the time of my last research upon the etiology of pseudo-membranous conjunctivitis[9]. Here I found the most beautiful branchings in cultures grown upon both egg albumen and washed-out blood fibrin. This branching must be interpreted as the expression of characteristic processes of growth; and that these morphological peculiarities are manifested in like manner by both species of bacteria, appears to me a proof of mutual relation. These branchings in the non-virulent bacilli have also been observed by Kanthack and Babes[10].

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[7] Berl. klin. Woch., 1896, No. 12.

The more one studies the pseudo-diphtheria bacillus, the more convinced he is of its great similarity to the genuine diphtheria bacillus; and at present there is no reliable means of differentiation other than the test of virulence. By this we do not, of course, mean to say that both bacilli must be identical; any day may bring us unquestionable proof that both bacilli are different, and but members of a natural family; at present, however, this family has by no means been definitely recognized. So long as this state of affairs exists the bacteriological diagnosis, especially when it is conducted without reference to virulence, can hardly have any great value, particularly in a diagnosis which is made within twenty-four hours.—American Medico-Surgical Bulletin.

THE SPECIFIC ACTION OF QUININE IN MALARIA.

Dr. E. C. Register, editor of the Charlotte Medical Journal, read a paper with this title before the North Carolina Medical Society.

After many years of study, both clinical and microscopical, the Doctor arrives at the following conclusion in reference to the specific action of quinine in the continued forms of malarial fever. He says a malarial fever without complications will subside after the plasmodia of malaria disappears from the blood; that we have in quinine the means to completely eradicate malarial poison from the body; that malarial fever occurring in a previously healthy subject, and in the central United States, if at once recognized and properly treated, never ends in death; that it is speedily curable, never continues, provided the nature of the disease be recognized and appropriate treatment employed.

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[8] Centralbl. f. Bact., XX, p. 1.

[9] Knapp and Schweigger, Arch. f. Augenheilk., XXXIII, p. 224.

[10] Centralbl. f. Bact., XX, p. 296.

Dr. Register has made microscopical examinations of the blood of several hundred patients suffering with remittent malarial fever, and has studied closely and thoroughly the crescentic and ring-shaped bodies which he says are the forms of the parasite which is responsible for the continued types of this fever, and he finds that the reason quinine does not always effect these irregular forms of the poison, is on account of the usual defects in its administration. He contends that the drug is very imperfectly absorbed when given by the stomach, and when the patient has a temperature of over 102 degrees. He says that in cases of continued malarial fever, if distinct and well marked intermissions of the fever are produced artificially by the use of antipyrine, antifebrine and phenacetine, the crescentic and ring-shaped bodies will disappear after the administration of quinine, as quickly as the spherical bodies that are found in an ordinary case of intermittent fever. In reference to the belief that the forms of the parasite that inhabit the blood cells are not acted on by quinine, he says: "There is no doubt in my mind that this belief is not erroneous. Besides my own observations, I have been able to collect the opinions of thirty-two authors touching upon this point, and twenty-eight out of the thirty-two believe that the endo-globular or intra-corpuscular forms are not, on this account, the cause of an uncontrollable fever, and that its proximity to the blood cell does not, in any way, protect it from the action of quinine."—St. Louis Medical Era.

Notes on the Treatment of Fæcal Fistulæ, abstracted from the Medical Record of October 24, 1896.—At the thirteenth annual meeting of the New York State Medical Association, which was recently held in New York City, Dr. Frederick Holme Wiggin, of New York county, presented a paper with the above title. The chief cause of the occurrence of fæcal fistula was stated to be the delay in resorting to operative measures, to which patients suffering from typhlœnteritis, or strangulated hernia were frequently subjected while their ailment was carefully diagnosticated. The view recently advanced by a writer on the subject under consideration, that the best treatment for this condition consisted in its prevention, was concurred in. But in the case in which this mishap had occurred, it was pointed out that if the opening was of small size, was located near or below the ileo-cæcal valve and no obstruction to the fæcal current existed, operative measures might be deferred, as in most instances the opening would close in a short time spontaneously. On the other hand, if the bowel opening was of large size, was situated laterally, or some distance above the ileo-cæcal valve, and was accompanied by the escape of a large proportion of the contents of the bowel, operative procedure for the closure of the opening should be speedily undertaken.

The histories of three cases, successfully treated by surgical measures, were cited. In two instances, the patients were inmates of the Hartford (Conn.) Hospital, and were operated upon by Dr. Wiggin, by reason of an invitation which was extended to him by the medical board of that institution, after several previous unsuccessful efforts to close the bowel openings had been made. The occurrence of the fistulous opening was due in the first case to failure, and in the second case, to delay in resorting to surgical treatment of typhlœnteritis, from which disease both patients originally suffered. In the third case, the bowel opening was caused either by the pressure of the gauze used to drain the abscess cavity, or by an ulcerative process which originated from within the gut. In the first case, as the opening in the bowel was of large size, irregular in shape, and the gut was thickened and friable, the diseased portion of the bowel containing the opening, about four inches in length, was excised, and the divided ends joined by the suture method of Maunsell. In the second and third cases, the bowel openings were situated in the head of the colon, and were in both instances closed by means of several rows of sutures, after which the omentum was drawn over the former site of the fistula, and retained by the sutures.

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PROCEEDINGS OF SOCIETIES.

CONGRESS OF AMERICAN PHYSICIANS AND SURGEONS.

FOURTH TRIENNIAL SESSION.
Held in Washington, D. C., May 4, 5, and 6, 1897.
WILLIAM H. WELCH M.D., of Baltimore, President.

AMERICAN DERMATOLOGICAL ASSOCIATION.

SECOND DAY—WEDNESDAY, MAY 5th.

In the first session of the second day's proceedings new officers and members were elected. The former were: President, Dr. Hyde, of Chicago; Vice-President, Dr. Bronson, of New York; Secretary and Treasurer, Dr. Bowen, of Boston (re-elected). The new members taken in were Dr. Montgomery, of Chicago, and Dr. Hutchins, of Atlanta. It was decided to meet next year upon the first Tuesday in June in or near New York city.

Dermatitis Due to X-Rays was one of the questions discussed in connection with the exhibition of photographs and drawings. Among the now rather frequent ill effects noted, aside from the more superficial lesions, are deep ulcerations, periostitis, and ostitis. Cases were reported by Dr. Gilchrist and Dr. Bronson. In some instances there is a rather deep indolent slough, which takes a long time to separate and is followed by very slow healing. The theory of bombardment by minute metallic particles was the one favored, and suggestions for prevention by the interposition of some non-penetrable screen were made. Dr. Gilchrist stated that those subjects who show after the first exposure any idiosyncrasy in this respect should be exposed a second time only in the most cautious manner.

Hereditary and Continuous Shedding of the Fingernails.—Dr. Douglass W. Montgomery, of San Francisco, reported the case of a strong healthy man of thirty-five years of age, who had been troubled from birth with a constant shedding of the fingernails. He stated that his mother's nails fell like his own, one or two nails being affected every seven or eight months. He had two maternal uncles whose nails fell every three years or so. Other members of the family on both sides had bad nails, but the patient could not say that they fell off. His immediate family had no affection of the nails. There were no stigmata of hereditary syphilis, no indications of psoriasis or eczema, nor of the generalized exfoliative diseases of the skin, nor of any constitutional disease whatever. There was a small quantity of albumin in the urine, but the patient did not appear to be suffering from Bright's disease. The patient said that one or two of his nails were constantly falling, six to eleven months completing the cycle. A nail first became a dull yellowish-white over the lunula, and then began to lift away from the nail bed from behind forward till it fell off. The shedding was completed in about three months, after which the nail grew in again perfectly normal in from three to eight months. There were no subjective symptoms attending the process, and there was no order in which the nails were attacked.

Dr. White asked if there were any trophic changes noted in the hair, or if other members of the family had been similarly affected. He referred to photographs in the exhibition room, which he had brought to illustrate how combined trophic changes in the hair and nails ran through three generations of a family.

Dr. Klotz spoke of similar changes affecting the teeth.

Dr. Montgomery said that the theory of degeneration would not apply. There were no changes in the hair or teeth; the nails were healthy in appearance and were shed unchanged. The patient's mother and maternal uncle had been similarly affected, while the father's nails had been reported as being "crumbly."

Symmetrical Atrophy of the Skin.—Dr. J. A. Fordyce, of New York city, reported a case which occurred in a woman, aged forty years. The hands, elbows, knees, and ankles were the seats of atrophic lesions, which were surrounded by a dusky-red zone of dilated capillaries. The atrophy followed closely on the hyperæmia, which seemed to be the primary condition. Clinically the affection resembled the cases described by Buchwald, Pospelow, Touton, Bronson, and others. The eruption was absolutely symmetrical, and had up to a short time before been progressive. On microscopic examination the atrophy was found to be secondary to an inflammation which was located in the skin and underlying connective tissue. The epidermis was invaded by exudation corpuscles, which had produced a thinning and degeneration of its constituent cells. No organic affection of the nervous system could be detected, although there was evidence of vasomotor disturbance.

Dr. Klotz spoke of a case which seemed to have originated in rheumatism.

Dr. Hyde thought some of the appearances were very suggestive of the erythematous form of lupus.

Dr. Gilchrist approved the term idiopathic atrophy.

Dr. Bronson thought there might be some central nerve lesion or possibly a spinal affection behind the process. While similar to his own case, it differed in being more diffuse.

Dr. Duhring said the condition was properly classed with the atrophies. Other diseases belonging in the same class, such as that known as striæ atrophicæ as well as morphœa, were preceded by inflammation. The latter, however, occurring in one stage of a disease, did not of necessity throw the affection out of this class, since in subsequent stages all inflammatory symptoms disappeared.

Some Cases of Feigned Skin Diseases.—Dr. F. J. Shepherd, of Montreal, read the paper. The first case was of a woman who was employed in a hospital, and who had patches of dry gangrene on both hands and forearms. The patches were about one inch in diameter and circular. They had been coming out for ten days before being seen by Dr. Shepherd. The hands were carefully bandaged with a starch bandage over cotton wool, and no more spots appeared. The girl rapidly got well. Some of the patches showed an appearance of concentric rings, and were evidently produced by a heated metal disk.

The next case was one of huge blisters on both cheeks of a woman, aged fifty, a cook on a farm. The blisters always came out after working in the fields—a proceeding to which she strongly objected. The blisters fused together, and one blister would cover the whole of one cheek. Some acid no doubt produced the lesions.

The third case was one of persistent croton-oil eruption after a single application two years previously. This patient, when the rash on the cheek faded, had an inflamed face, followed always by the development of huge blisters on each cheek. The patient was a young woman of a decidedly hysterical temperament, and seemed to take delight in showing her breasts covered with croton-oil eruptions to the students.

The fourth case was one of patches of gangrene on the dorsum of the left foot. This was the third attack in four years. The first followed recovery from typhoid fever. As soon as the gangrenous skin separated, the ulcer left behind soon healed. The girl liked being in the hospital, where she attracted some attention, owing to the peculiarity of her case and the absence of cause, as the patches did not affect any one area supplied by a special nerve, and the ends of the toes in which the circulation was good were quite normal. The girl was addicted to liquor and tobacco. On one occasion in the hospital she set fire to her clothes. Dr. Shepherd drew attention to the similarity of these gangrenous patches to those occasionally produced by hot-water bottles applied to unconscious patients. The absence of motive in a case would not preclude the diagnosis of a feigned disease. A desire to obtain sympathy or notoriety is sufficient cause in many cases.

Dr. Allen believed that we should make a distinction between hysterical eruptions and those intentionally produced. He referred to an instance of multiple gangrene as belonging with the first class. He spoke of the great importance of differentiation between these conditions and burns from hot-water bottles closely simulated in one of the reader's photographs.

Dr. Duhring agreed as to the importance of excluding hysterical eruptions, and thought that before we can call an eruption "feigned" we must have absolute proof that it is such. He had personally observed but few instances of feigned lesions, and had proven in numerous instances that cases which had been so designated were in reality of neurotic origin.

Dr. Grindon spoke of artificial eruptions which were not feigned, instancing a young man who inflicted burns upon the skin as a sort of penance.

Dr. Bronson spoke of the morbid susceptibility on the part of the patient, especially in the matter of hot-water-bag burns, and thought the reduced condition of the patient had much to do with the severity of the traumatism. Similar lesions may occur not so much by design as because of some peculiar exaggeration of reaction to slight injury.

Dr. White could not agree with Dr. Duhring that absolute proof was required. It would rarely happen that one could be detected in the act of self-mutilation. In convictions for murder, circumstantial evidence may have been the strongest if not the sole proof. Why should we not admit it here? He referred to an ulcerative dermatitis about the region of the mouth, evidently due to some application for removing hairs, but the use of all such applications was most strenuously denied by the patient.

Impetigo Herpetiformis.—Dr. M. B. Hartzell, of Philadelphia, reported a case of this nature in a woman over seventy years of age. An eruption of pustules upon the trunk, attended by chill and fever, was followed by similar lesions upon the face, neck, and extremities. The lesions were grouped, and followed by crusting with miliary pustules about the border. There were severe constitutional symptoms. There were several recurrences, and death finally resulted. Of the twenty-one cases reported three have occurred in men. Tuberculosis has been found in three of the seven instances in which autopsy was made.

Impetigo Herpetiformis or Dermatitis Herpetiformis.—Dr. J. A. Fordyce, of New York city, reported a case occurring in a man aged sixty-six. The eruption when first seen was nearly universal. It was made up of grouped vesicles and vesico-pustules, which became confluent over the legs and arms. Over the thighs and abdomen large irregularly rounded plaques were seen, which extended in a peripheral manner, while slowly clearing in the center. A new outbreak of lesions was accompanied by an elevation of temperature and intense pruritus. After existing for several weeks the eruption slowly disappeared, leaving larger pigmented areas on which new groups of vesico-pustules would appear. Papillomatous lesions developed in both axillary spaces. A small amount of albumin and some granular casts were found in the urine. The blood and serum from the vesicles contained a large number of eosinophile cells. A piece of skin from a recent lesion excised for microscopic examination showed an acute inflammation of the derma and the presence of vesicles in the deep layers of the epidermis. The cell exudation contained large numbers of polynuclear leucocytes and numerous eosinophile cells. The eruption presented certain features pertaining to both impetigo herpetiformis and Duhring's disease, and seemed to justify the view which this writer originally held, that they should both he included in the same class.

Dr. Duhring said that in Dr. Hartzell's case he had been impressed by the thickening of the skin from chronic œdema, considerable corium being involved. The grouping was irregular. There would seem to be a close connection between this disease and pemphigus vegetans, to which it bears marked analogy. The mucous layer of the corium is thickened in both. Dr. Fordyce's case should be ranged with those of impetigo herpetiformis, in which there is marked herpetiform arrangement, but the lesions are deeper seated than in dermatitis herpetiformis. He thought it well to retain the term proposed by Hebra for this class.

Dr. Johnson said he had seen two cases, one in a male subject. The condition of the skin described by Dr. Duhring as soft and leathery was not present in these cases. He regarded Dr. Fordyce's case as dermatitis herpetiformis, among other reasons because there was a vesicular eruption.

Dr. Gilchrist thought bacteriology offered much better chances than clinical observations in arriving at the etiology of these affections. Eosinophiles are found to be present in a variety of vesicles, including those due to poison ivy.

Dr. Hyde said eosinophile cells were present in abundance at times in the lesions of dermatitis herpetiformis.

Dr. Hartzell said that in his case the grouping was marked, but the distinctive feature was the surrounding ring of miliary pustules.

Dr. Fordyce said a peculiarity in his case was an entire absence of scarring, thus differing from dermatitis herpetiformis. He agreed that the two conditions were closely related to each other and also to pemphigus vegetans. A tendency to papillary formation is common to all these conditions.

Dr. Duhring said Dr. Fordyce's observations relating to scarring differed from his own. He did not recall any case of permanent scarring in uncomplicated dermatitis herpetiformis.

THIRD DAY—THURSDAY, MAY 6TH.

Strong Applications of the Ichthyol Group was the first paper of the second day's session, by Dr. H. G. Klotz, of New York. Ichthyol, thiol, and tumenol, members of the "reducing" group of drugs, are usually applied only in weak combinations in skin diseases. The reader had found by experience that strong applications are very effective even in acute inflammatory conditions of the skin, if judiciously used at the proper intervals. Besides the effect on cornification, the contracting power upon the blood-vessels and the anti-parasitic properties came into play. The speaker laid particular stress upon the facts that ichthyol dries rapidly on the skin and forms a suitable protective without further dressing. He has used it besides in erysipelas, in which its value is widely known; in other forms of acute dermatitis, viz., dermatitis venenata, with particularly good effects in dermatitis from burns and from freezing; in traumatic dermatitis (mostly from intertrigo and scratching); further in acute eczema, herpes zoster, and he thinks it will be found useful in erythema, multiforme, dermatitis herpetiformis, urticaria, and in the acute exanthemata. In chronic inflammations the effect is more pronounced if preceded by cauterization or strong stimulation.

Discussing Dr. Klotz's paper the speakers recognized the value of ichthyol in many affections and thought well of the suggestions as to modes of application. Tumenol was stated to be more efficacious in relieving pruritic affections. It is claimed by Bronson that stronger solutions of bichloride can be used with advantage when combined with ichthyol than in watery solution. Since ichthyol in strong watery solution dries into a varnish-like coat, as has been shown by Klotz, the futile attempts to find a suitable medium for its application as a protective dressing may be abandoned. It was thought, however, that in certain conditions, such as erysipelas, collodion might in itself exert a beneficial influence upon the dilated vessels, and thus aid in the reducing and antiparasitic action of the drug.

The Nature of the Xanthomata was the title of a paper presented by Dr. S. Politzer, of New York. The peculiar yellow plaques and nodules in the skin known as xanthoma have been the subject of extensive studies on the part of pathologists and dermatologists, ever since they were first described by Addison and Gull in 1850. The greatest diversity of opinion exists as to their nature. This is due probably in part to the fact that different authors have examined different stages of the disease, and in part to the fact that the different forms of xanthoma have been assumed to be merely different clinical manifestations of the same process. The author's histological studies are based on thirteen cases—five of them cases of xanthoma planum palpebrarum, four of xanthoma tuberosum, and four of xanthoma diabeticorum. The clinical grounds for separating xanthoma of the eyelids from multiple xanthoma are as follows: The nodules of xanthoma multiplex are firm, round, elevated papules; the patches of eyelid xanthoma are soft plaques in the level of the skin. Eyelid xanthoma persists through life; multiple xanthoma sooner or later undergoes involution. Eyelid xanthoma is quite common; multiple xanthoma is extremely rare. If the eyelids were in this preponderating degree the seat of predilection for a common xanthoma, we should expect to find the eyelids affected in every case of multiple xanthoma; but, as a matter of fact, the two forms are rarely associated in the same individual. With the extensive material at his command the author has been able to show that common eyelid xanthoma is not a new growth, but is due to a degeneration of pre-existing, embryonally misplaced muscle tissue. The so-called xanthoma cell is a fragmented muscle fibre in a state of granulo-fatty degeneration, with proliferation of the muscle-cell nuclei. The various stages of the change from normal muscle fibre to "xanthoma cell" were demonstrated in sections under the microscope, drawings made by Dr. Ira Van Gieson, and microphotographs. This explanation of the origin of eyelid xanthoma harmonizes with a number of hitherto unexplained clinical and pathological facts, e. g., the absence of any clinical signs of tumor; its almost exclusive occurrence in the face, where peculiar muscular conditions prevail; its heredity; its usual development after middle age when degenerative processes are apt to occur; the peculiar yellow pigment that is always present in muscles undergoing fatty degeneration, etc. The structure of multiple xanthoma is shown to be wholly different from that of eyelid xanthoma. It forms a sharply circumscribed tumor in the cutis. It is an irritative hyperplastic development of connective tissue, whose cells produce fibrous tissue on the one hand, or undergo fatty degeneration on the other. In diabetic xanthoma the process is a little more diffuse and the tendency toward fatty degeneration more marked than in the non-diabetic multiple xanthoma. In both, irregular patches of granulo-fatty matter, interspersed with cellular detritus, occur in the middle of the nodules as the results of the fatty degeneration of the cells. In over eighty-five per cent. of the recorded cases of multiple xanthoma, far too large a number to be accounted a mere chance, there was either diabetes or some severe lesion of the liver with jaundice. The author thinks it likely that further research may show that the fibrous nodes and fusiform enlargements of tendons in chronic rheumatism are to be placed in the same general class as the nodes of xanthoma. We should then have a large group of diseases, hepatic, diabetic, rheumatic, all characterized by toxæmic conditions, in all of which irritative connective tissue lesions occur in the skin and elsewhere. At one end of this series we should have the persistent fibrous node of rheumatism; at the other, the transient nodules of diabetic xanthoma, while between them, intermediate in its tendency toward the formation of fibrous tissue and fatty degeneration, ultimately undergoing involution, would stand the nodule of common multiple xanthoma.

Discussing this paper Dr. Hyde said that while it was unusual for xanthoma planum of the lids to occur in diabetics, he had observed such an instance. He agreed with the reader that there were objections to the qualitative "diabeticorum" for the variety to which it was applied.

Dr. Johnson said the fact that the lesions in multiple diabetic xanthoma might disappear coincidently with a decrease in the sugar of the urine, and recur with a renewed excess of the latter, plainly took these lesions out of the class of new growths. Xanthoma of the lids could, therefore on this ground alone, be separately considered.

Dr. Morrow thought there was a direct casual connection between the presence of sugar in the urine and the development and exacerbation of lesions in the diabetic variety. Lesions tend to recur after removal, as he had shown in a case of his own.

Dr. Allen said that at the last meeting he had advocated in a paper on "glycosuric dermatoses" that the term "diabeticorum" be dropped in describing several affections occurring in the presence of the symptom glycosuria.

Dr. Bowen thought the reader's theory of the formation of the xanthoma cell was very likely to be correct. It was greatly to be desired that transition forms should be demonstrated to strengthen the reader's claims.

Dr. Shepherd reminded the members of the case he had presented in Montreal, in which the lesions of xanthoma multiplex had promptly disappeared upon the removal of a mass of calculi from the gall bladder.

Dr. Politzer closed the discussion by stating his belief that the generalized form of xanthoma occurring upon the hands, face, etc., was of the connective-tissue variety, and that hence it was capable of disappearing spontaneously. Glycosuria in patients with eyelid xanthoma was a coincidence only. Behind the sugar there probably existed a toxæmia acting in a causative way to produce the multiplex form called "diabetic."

The Dermatologist in Pathology.—Dr. Welch, president of the congress, was present at this session and made a brief address, in which he referred to the opportunities which were being grasped by those working in cutaneous medicine for the elucidation of complex observation and experiment. The skin more than other tissues is subject to a great variety of external irritants and infections whose reactionary effects can be closely watched in the living subject and excised for microscopic study. Properly pursued investigations in this department he confidently expected would in the near future add enormously to our knowledge in the broader realm of general pathology. He expressed much personal satisfaction with the section's work.

A Universally Adaptable Scale of Measurements for the Accurate and Uniform Description of Cutaneous Lesions.—This paper was presented by Dr. C. W. Allen, of New York, who showed first the desirability of a system which could be adopted for all countries. The inaccuracy of description by reference to seeds, grains, nuts, fruits, vegetables, coins, and the like was gone over, and the international confusion often occasioned in important questions was dwelt upon. The scheme proposed took for the unit one-quarter of a millimetre, suggestively called by the reader a *tetmil*, from the Greek equivalent indicating "one-quarter." The sizes range from one to five hundred, and a series of numbered circles inclosed in the larger circle of the scale show at a glance their relative size. It was suggested to authors to insert the scale in their text-books on dermatology, and that by its use the writer would be saved long descriptions and the reader would soon familiarize himself with the numbers and secure a more adequate idea of the size and form of lesions than by the older method. Scientific accuracy was, however, the desideratum, especially as to the smaller lesions, upon whose exact size a diagnosis often rested. A table of equivalents giving the old terms and their corresponding scale size was incorporated in the paper, based upon actual measurements, and it was thought by the reader that this feature alone, if adopted, would prove of value as giving to the various objects now employed for comparison a definite dimension based upon the average sizes of objects which often varied greatly as between themselves. The proposed scheme was discussed and it was generallly admitted that reform in the direction proposed was called for. It was voted to refer the matter to a committee which should bring the question up at the next international congress.

A Peculiar Affection of the Hair Follicle was the paper next read by Dr. Joseph Grindon, of St. Louis. The affection consists of a chronic inflammation of the hair follicle characterized by extrusion of a portion of the root-sheath *en masse*, which remains threaded over the hair and is carried up with it in its growth, the process being repeated from time to time until successive masses bearing a superficial resemblance to nits are strung along the hair. It is accompanied by slight redness about the mouth of the follicle and results in a curable alopecia. There are found little grayish-white masses threaded beadwise on the hair. In their size, color, and distribution they are so like the ova of the pediculus capitis that one is almost certain to mistake them for the latter at first sight. The little masses consist of the cells of both layers of the internal root sheath (Huxley's and Henle's) surrounded by a layer of amorphous detritus and sebum. Besides pediculosis there are other clinical conditions which superficially stimulate this form of folliculitis. Among them are trichorrhexis nodosa, monilethrix, piedra, leptothrix, and a few clinical curiosities, such as Hodara's disease, Giovannini's disease, etc. In the discussion this observation was considered of interest and importance in connection with nomenclature of the diseases of the hair, as well as in differential diagnosis.

Parakeratosis.—Eleven cases of parakeratosis (Mibelli) or hyperkeratosis excentrica (Respighi) in one family were reported in a paper by Dr. T. Caspar Gilchrist, of Baltimore. "These two patients, father and son, whom I am able to exhibit to-day, are two of the eleven cases of this very rare disease. The first patient whom I have named in the scheme which I present below I have called " the patient," as he was the first of the cases. The father, who is sixty-four years old, shows almost all the typical lesions of this disease, especially on the backs of both hands, fingers, wrists, forearms, and palms, as well as on the legs and dorsum of the feet. The patient is a young man, twenty-one years of age, who has had the disease since he was five years old, when it commenced on his ears, nose, chin, neck, back of hands, and right forearm. The lesions were extremely slow in appearing and in growing. The most remarkable feature about the whole case is the family history, which is here given in a tabular form, together with a very brief description of the disease in each case.

Grandmother——*One Brother*
(disease on the face; now dead).
(lesions on the face commencing at an early age and lasting until death).

Father——*One Brother*
(disease began at about 8 years of age and is on the face, palms, back of hands, forearms, legs, feet, and sole of one foot).
(disease commenced at about 9 years of age; on face and leg; died at 72).

One Sister——PATIENT——Brother (1)——Brother (2)——Brother (3
(28 years old, married; disease commenced at 7 years; only on face).
(aged 20; four lesions on nose, commenced at 5 years.)
(aged 17; disease commencing[?], one lesion on neck).
(aged 15; number of lesions on face: began at 9).

Son——*Daughter*
(9 years old; disease commenced at 7 and quite extensive on the face).
(aged 11; disease commenced at 9 years; on face).

"There have been eleven cases in four generations, eight males and three females. Of these, eight had been examined personally. The separate descriptions given by three of the family of the disease as it occurred in the others, whom I examined later, were so correct that I have fully accepted the statement that it was the same disease which occurred on the deceased members. The patient (first) has been under my care eighteen months, and the disease was not diagnosed until many histological specimens were examined. The eruption consists of lesions of various sizes and forms, which present two varieties, viz., the smallest, which are chiefly on the face, and consist of minute (0.5 to 1 mm. in diameter)), dirty brown, semiglobular elevations of a horny nature. They grow very gradually, and when they reach the size of a small pinhead (1 to 2 mm.) the centre sinks in and a peripheral horny ridge is found. Coursing along the ridge appears a minute groove, and as the patch becomes large the centre takes on an atrophic though callous appearance. The largest patches are about split-pea sized. In the father's case there was absence of sweating within the areas of the largest patches. It was found that after removal of some of the lesions with a curette and the application of the silver-nitrate stick, the characteristic papule would reappear on the scar. They did not return after excision. New lesions had been seen to appear during the course of weekly observations of the patient. Microscopical examinations were made of sections obtained from twenty lesions excised from the patient, three from the father, two from one of the brothers, and one from the nephew; and the result showed that the disease began either as a marked hypergeratosis of the sweat pore and upper portion of the duct, or of the opening of the sebaceous gland, or of the two adjoining openings of both glands. This was followed in some instances by dilatation of the sweat duct and gland beneath, followed by atrophy of the glomerules. Sections from some of the oldest lesions presented a picture almost like that of psorospermosis follicularis vegetans (Darier).

"Mibelli has reported three cases and very briefly referred to three others in Italy; Respighi reported seven cases of this disease and also briefly referred to four others, but only one case has been published outside of Italy, and that was by Hutchins, of Atlanta, Ga. (He gave only a clinical description.) Neither Mibelli nor Respighi thought that this disease was hereditary, but these eleven cases in four generations show very strong evidence of an hereditary taint.

"The reader proposes the name porokeratosis excentrica for this disease, as the former refers to the chief pathological factor, and the latter to a very striking clinical feature."

What Conditions Influence the Course of Syphilis? was the subject for general discussion, which was opened by Dr. R. W. Taylor, of New York, who presented the adult aspects of the case. The subject of infection and course of syphilis was exhaustively reviewed, and the conclusion reached that a great variety of conditions modify the effects of the virus in different individuals.

Infantile Syphilis.—Dr. James Nevins Hyde, of Chicago, opened the discussion on "Infantile Syphilis—What Conditions Influence its Course? (a) The Virus? (b) The Individual?" He considered, first, the phenomena of syphilis in intra-uterine life, syphilis of the ovum and of the fœtus; second, extra-uterine or hereditary syphilis of the infant; third, acquired infantile syphilis. With reference to the first, the author considered its frequency in several social states of the people, the errors most commonly committed in the interpretation of the signs of the disease, and the percentage of abortions and miscarriages in the products of conception. Collating the fruits of seventeen hundred pregnancies, it was shown that about one-third terminated by the death of the ovum or fœtus. With reference to the facts of extra-uterine syphilis of the products of conception, after a review of the sources of error, it was shown that of seventeen hundred pregnancies there were born eleven hundred and twenty-one living children, of whom nine hundred and fifty-six died within the first twelvemonth after the conclusion of pregnancy. Considering, then, the relative importance of both virus and individual, the struggle for existence would appear to be hopeless were it not for a single and remarkable fact. The mothers who can offer breast milk to their syphilitic children save between ten and twenty per cent. of the patients who are set down by some authors as inevitably lost if not nourished at the breast of the mother. This remarkable result is due and solely due to the improved nutrition of the victim of the disease. Acquired infantile syphilis was shown to result in an enormous proportion of all cases with favorable results as regards the new being, the author citing a number of illustrative facts, and concluding that in these cases, as in acquired disease of adults, "he is indeed blind who cannot see . . . the wall that is ever and again built as a bulwark to the encroachments of disease by the sound constitution, the satisfactory habits, and the hygienic environment of the selected individual."

In the discussion, Dr. Atkinson took the ground that the intensity of the poison depends largely upon the susceptibility of the individual.

Dr. Grindon thought the dosage a matter of importance which had not been touched upon, and spoke of certain observations which had been made in connection with vaccination, which went to show that the amount of virus inserted at times influenced the result.

Dr. Hartzell thought it largely a question of soil, and that bacteriology substantiated this view in relation to many similar processes.

Dr. Allen thought there were two sides to the question, and that reasoning from analogy we must admit an occasional intensity of virus—as in a succession of nerve and cerebral cases.

Dr. Montgomery spoke of superinfections, and objected to the use of the term symbiosis in this connection.

Dr. Klotz called attention to his own paper, which had not been referred to, but which went over the whole question. He said his own conclusions had been as hypothetical as those arrived at by the speakers.

Dr. Fordyce had confirmed the observation of Van Gieson, that secondary infiltrations occur at an early period (a few days) at some distance from the initial sore. In gouty subjects syphilis is apt to be severe—the late sclerosis may be a combination of a lithæmic state superadded to the specific virus effects.

Dr. Sherwell agreed with Dr. Klotz that the germs of tuberculosis and syphilis are antagonistic, and does not believe that syphilis leads or predisposes to tuberculosis.

Dr. Morrow held that the soil and not the seed determined the nature of a given case. Analogy does not help us in the solution of the problem. We must rely upon clinical data. In hereditary syphilis we must assume that in succeeding pregnancies there has been a successive attenuation of the virus.

Congenital Ichthyosis.—Dr. James M. Winfield and Dr. J. M. Van Cott presented conjointly, "A Contribution to the Etiology of Congenital Ichthyosis, Report of a Case with Absence of the Thyroid, with Microscopic Report of the Condition of the Skin." After giving the family history, the paper entered into the history of the mother. She had been pregnant seven times, and four times gave birth to normal children; two were ichthyotic, the first of which was reported by Dr. Sherwell before the American Dermatological Association in 1894. During the first, second, third, fourth, and sixth pregnancies the mother's condition was excellent, she being neither nervous nor sick. During the fifth, which resulted in the first ichthyotic infant, she was subjected to considerable anxiety, and at the sixth month received a severe nervous shock. When she became pregnant for the last time she immediately grew exceedingly nervous and worried lest she should become the mother of another deformed baby.

At delivery the infant weighed about four pounds. The body was enveloped in what appeared to be a thick coating of vernix caseosa, which on removal left the skin red and shiny. If no lubricating protective was used the whole cutaneous surface soon became scaly and fissured. Four days after delivery it appeared as follows: the whole body was covered with thick reddish-brown epidermic plates, which were larger and more marked over the extensor surfaces; some of them were from one-sixteenth to one-tenth of an inch in thickness, and one-half to two inches in diameter; between the plates were fissures of varying depths. Movement of the limbs produced cracks about the flexures of the joints, which extended deeply into the underlying tissues. The autopsy was made about eight hours after death. The body was considerably emaciated, and covered with large fatty epidermic scales. On examining the neck the thyroid gland was found to be absent. There was no sign or trace of its ever having existed.

A number of slides were prepared from portions of the skin taken from the back, thighs, arms, and scalp. Dr. Van Cott's report was: "The horny layer of the skin is considerably thickened; the rete Malpighii appears fairly normal. The sweat and sebaceous follicles are present. Both the rete and subcutaneous areolar tissue present in some of the sections evidences of necrosis with small round-cell infiltration, and in certain areas the lymph spaces are occluded with bodies resembling in every respect micrococci. Those lymph spaces, which are filled with these morphological elements are more common in the neighborhood of the blood-vessels, both arterioles and venules.

"The important etiological points are: First, the absence of the thyroid. Second, the presence of micro-organisms in and about the lymph spaces. That the thyroid has some influence on the nutrition of the skin is an established fact, as is evidenced by the occurrence of various dermatoses when this body is atrophied or diseased; and, further, there are numerous recorded instances in which the administration of thyroid extract has exerted a beneficial influence on skin affections when hyperkeratinization and thickening were prominent features. Just what etiological bearing the absence of the thyroid has on the production of ichthyosis congenita and kindred diseases cannot be positively stated without further clinical and post-mortem proof; still from the above facts it is but fair to conclude that in this case, at least, the absence of this body has some marked effect on the nutrition of the skin and the development of this cutaneous affection. The occurrence of micro-organisms in the lymph spaces at once brings up the question whether this and similar cases are types of true ichthyosis, or, as Dr. Van Cott puts it, a variety of dermatitis. If these micro-organisms were the cause of this excessive plate formation, it is possible that the term ichthyosis sebacea is the correct one; for a bacterial irritation could easily produce a universal seborrhœa."

Pseudolupus of the Face Due to the Presence and Growth of Blastomyces was the title of a paper presented by Dr. T. C. Gilchrist, of Baltimore. A photograph was passed showing an extensive lesion of the face, which had evidently destroyed the eyelids to a great extent and caused marked disfigurement. The objective appearances were those of a lupus vulgaris, or of a tuberculosis of the skin. The affection had begun a number of years before as a papule near the ear, from which point extension had gradually occurred, leaving scar tissue to mark its path. A plaque covering the back of one hand had existed for four years. A chart was exhibited showing a series of inoculation experiments in animals, which resulted in nodules in the lungs, from which cultures were made and other animals inoculated. In every instance pseudotubercle postulation or caseation was the result, and which by inoculation would always produce lesions similar to those of tuberculosis, which latter disease was, however, always most carefully excluded. The bodies which produce this blastomycetic dermatitis are doubly contoured, budding organisms, resembling the yeast plant and growing in the same way. They have never before been shown to possess interest to the dermatologist. They were demonstrated under the microscope.

Lymphangioma of the Labia Majora was the title of a paper presented by Dr. J. C. White, of Boston. The affection had existed for several years in the person of a young lady, and assumed the form of papillomatous growths resembling lymphangiectases as much as they did lymphangioma. The diagnosis was difficult, and the reader did not know in just what way it was fully to be determined. Drawings were shown and the microscopic appearances demonstrated.—Medical Review.

THERAPEUTICS.

THE TREATMENT OF OPIUM POISONING BY PERMANGANATE OF POTASSIUM.

The British Medical Journal of May 16, 1896, contains an article with this title by Maynard, who states that while officiating as resident physician at the Medical College Hospital, Calcutta, he had unusual opportunities of seeing and treating cases of opium poisoning. He employed the treatment in nineteen cases, the following solutions being used:

No. 1 solution:

℞	Potass. permang	gr. x,
	Acid. sulph. dil	ʒ ij,
	Aq	O j.

As this does not keep well, the sulphuric acid has to be kept separately, and the quantities in the stock mixtures are so arranged that two drachms of the permanganate and two drachms of the sulphuric acid in concentrated solution, added to a pint of water, provide the proper amount for use.

No. 2 solution:

℞	Potass. permang	gr. ij,
	Aq	O j.

This is also kept in a concentrated stock mixture, and, though used as here given, would probably have acted better if mixed with sulphuric acid like No. 1.

No. 3 solution:

℞	Potass permang	gr. j,
	Aq	♏ xx

For hypodermic injection.

At first the treatment was carried out as recommended by Dr. Moor (British Medical Journal, June 22, 1895)—namely, by introducing two or three strong acidulated permanganate solutions into the stomach at intervals, and afterward weaker solutions at intervals for some hours, to oxidize any morphine that might be re-excreted into the stomach. It was found, however, that these solutions, if allowed to escape through the stomach tube, returned discolored, and as this indicated organic matter (or opium) remaining, it was soon decided to wash out the stomach with weaker permanganate solution until it returned pink; and the following instructions were finally issued to the house physicians regarding the use of the stock solutions:

1. Wash out stomach twice with plain water, keeping washings for chemical examiner.

2. Wash out stomach with No. 2 solution until washings come back pink, retaining the solution in the stomach one minute each time.
3. Then introduce No. 1 solution, and leave it in the stomach.
4. Repeat No. 1 solution twice, at intervals of half an hour.
5. Then give No. 2 solution every half-hour until complete recovery.

The ordinary stomach-pump tube has too small eyes to allow of the contents of the stomach being thoroughly washed out, and it is better to use a tube entirely open at the stomach end—an ordinary half-inch drainage tube answers the purpose. A pump, by means of which the solution can be forcibly injected, is better for dislodging the adhering opium than an elevated funnel, which only causes a gentle stream to enter the stomach.

The amount of permanganate required depends, of course, upon the amount of drug taken. The washings must be continued until the solution returns pink. Experimentally it was found, when permanganate was added to opium in aqueous solution and shaken, that the mixture ceased to give any reaction with pure nitric acid after 1.25 parts of permanganate had been added (taking the opium present at one part by weight); it ceased to smell of opium after the addition of 3.5 parts, and to be discolored after 10 parts of permanganate had been added.

The general effect of permanganate of potash does not appear to be poisonous. No reference to it is to be found in text-books on toxicology. One cannot imagine it being absorbed as such, and the hydrated dioxide of manganese is non-poisonous. Locally it is an irritant, and, if in strong enough solution, an escharotic. In no case, however, were any signs found, either after recovery or on post-mortem examination, of its having caused inflammation of the gastric mucosa. Much decomposed permanganate (hydrated dioxide), but no trace of inflammation, was found on post-mortem examination at the site of hypodermic injections, while other cases that recovered after hypodermic injections showed no ill-effects. Vomiting after the introduction into the stomach of permanganate solution may be taken as a good sign, as it indicates a return of excitability in the gastric mucosa, and is only met with during recovery.

Dr. Moor states that permanganate decomposes morphine sulphate infinitely more rapidly than albuminous matter, and that it selects the former when both are present; and in opium poisoning he gives dilute sulphuric acid or white vinegar with the permanganate, on the ground that the insoluble morphine will be thereby at once converted into the soluble sulphate of acetate. This can hardly be the true rationale, however, as, while morphine acetate is very soluble in water (1 in 12) and spirit, morphine sulphate is only half as soluble in water (1 in 24, the same as the hydrochlorate) and but sparingly soluble in spirit; and the explanation, if it holds for the acetate, will not do so for the sulphate.

The truer explanation is probably to be found in Lauder Brunton's statement (taken from Stoluikow) that "the activity of morphine appears to depend on the presence of hydroxyl (HO) in it. When this is replaced by SO_4, its activity is greatly diminished." The antidotal power of permanganate—such as it is—is probably due still more, however, to the oxidation of the alkaloids. To quote Brunton again: "In the alkaloids produced from morphine by oxidation (oxy-dimorphine and oxymorphine) their narcotic action is diminished without the convulsant action being increased." When given by the stomach with sulphuric acid, therefore, permanganate may be regarded as acting in three ways—(1) by substituting SO_4 for HO in the morphine present; (2) by oxidizing the morphine present; and (3) by mechanically washing out the opium present—and in his opinion its special value lies in that its discoloration (deoxidation) tells us readily whether any opium remains to be removed.

As regards hypodermic injection, Dr. Moor says: "While a part of the potash salt is reduced by the subcutaneous tissues, yet enough remains to find its way to the circulation to select the morphine out of the albuminous constituents of the blood." It is difficult to believe this is possible of a very readily deoxidized substance like permanganate of potassium, and the appearances in Dr. Maynard's post-mortems make it even less likely. What Brunton says of permanganate is quite applicable: "It very readily parts with its oxygen, and thus destroys completely the deadly power of the latter, and the mixture may be injected subcutaneously without any bad effects. When injected after the poison, however, it does not appear to come into such immediate contact with it in the tissues as to destroy it, and it therefore does not act as an antidote."—Therapeutic Gazette.

FORMALIN AN APPROXIMATE SPECIFIC FOR RINGWORM.

An interesting editorial note has appeared in Guy's Hospital Gazette, calling attention to a recent paper by Mr. Alfred Salter, on the treatment of ringworm by formic aldehyde, or formalin. This treatment is now so well known in Guy's, and has had such a conspicuous success, that it should be part of the ordinary practice of every old Guy's man. There seems no doubt that it is the almost specific treatment for the disease, especially in obstinate and hitherto incurable cases. And yet this discovery arose from the annoying fact that the inventor's cultivations of the ringworm microbe were all killed one night through his having left the stopper out of the formalin bottle.

MALARIA IN CHILDREN.

A. Zuckermann uses the following:

℞ Tinct. eucalypt. glob.
Spts. vini. dilut.................... aa 8.0
Quin. mur............ .... 2.0
Chinoidini............................ 1.25 to 2.0
Acid mur. dil., q. s. ad. solut.
M. Sig.—20 to 40 drops five times a day.

—Esheledelnik russ, 1895, 34.

TREATMENT OF LARYNGISMUS STRIDULOUS.

℞ Tinct. aconiti.
Tinct. bellad..................... aa gtt. x.
Aq. laurocerasi 15.0
Aq. aurant. flor..................... 60.0
Aq. tiliæ........................... 60.0
Syr. simpl. 30.0

M. Sig.—A teaspoonful every hour. For children two years of age a coffeespoonful is the dose.

Lemoine uses bromide of potash as follows:

℞ Potas. bromat. 15.0
Syr. chinæ.
Syr. cort. aur.................... aa 150.0
M. Sig.—Two teaspoonfuls daily (for three-year-old child).

During the spasm apply to the anterior surface of the neck a sponge dipped in hot water, as hot as the child will bear it. At the same time steam should be generated in the room, and if mucus has collected in the pharynx, it should be mechanically removed. Look for the cause of these attacks. Frequently adenoid vegetations will be found present in the naso-pharyngeal space and they should be removed.—Pediatrics.

CREOSOTE IN GONORRHEA.

Asmus (Meditzinskoie Obozrenie, No. 10, 1896,) reports fifty-eight male cases of acute gonorrhea successfully treated with injections of from two- to ten-per-mille emulsion of creosote. The discharge quickly decreased, became mucoid, and then ceased altogether. The patients recovered more rapidly than under the ordinary methods of treatment; complications developed but rarely, and no relapses occurred. In addition, creosote seemed to exercise an anesthetic action on the urethral mucous membrane.—British Medical Journal, July 11, 1896.

MISCELLANEOUS.

MEDICAL MEN AND THEIR HEALTH.

At this season when so many other portions of the community are planning vacations, relaxation, shorter working hours, it is well that the doctor, who spends all the rest of the year caring for the health of others, should take stock of his own and consider whether he is sufficiently solicitous for improving, or at least for keeping good, his own physical condition, for there is no laborer in any walk of life for whom such care is more essential.

Under the above head, Dr. John W. Teale, in the Lancet, offers some useful suggestions which physicians would do well to ponder. In response to a hypothetical question, "What will most conduce to the success of a general practitioner?" he replies, "Power of concentration and command of temper."

By power of concentration he would mean that power by which a man, however wearied, on entering a house is able at once to abstract his brain from everything that has already happened, and to concentrate his mental faculties on the case that is before him. Patients are naturally somewhat selfish and are very quick to observe if they do not get full attention, and if, when the finger is on the pulse, the mind is not on the patient.

Secondly, command of temper. To the quick, high-strung, sensitive man, exhausted by the worry and anxiety of daily life, thorough command of temper with testy, querulous, exacting patients can only be obtained by rigid self-control commenced in early life. To be forever bearing in mind that the patient is the sufferer, that testiness and ill-temper are due to physical weariness and distress, and not to disloyalty to the doctor, is a task that will try the strongest nature.

In order to cultivate and obtain this concentration and command of temper, the physician should live as far as possible a simple outdoor life in constant physical training. "Why," he asks, "should a man become stout and short of wind because he has reached fifty?" Simply because he is struggling with life work when his physical condition is not fit to grapple with it.

The exercises which Dr. Teale recommends are performed with light Indian clubs; these or with dumbbells or the ordinary parlor gymnastics constitute a kind of training which students are apt to drop when they leave college, but which can profitably be kept up during the whole life. It may be said that many medical men do not need these exercises, their physical powers being sufficiently tested every day on their bicycle and in other ways. Some in the country do a little at sawing or splitting wood in the morning to keep themselves in trim. It is a mistake to suppose that the busy man wants a great deal of physical exercise. His ordinary day's work, with what it involves in taxing mind and body, is generally enough for him. One writer, moreover, suggests that it is well for the physician to cultivate the art of sleeping for a few minutes at any time. A man can only live safely on the interest of his vital strength. Any withdrawal of principal should be promptly replaced.

Dr. Teale advises that two good meals and one moderate one be taken each day, and thinks that a late dinner is preferable, for if taken in the middle of the work, either the meal or the patients must suffer. It is possible to be too busy to dine, in which event a cup of soup, or a sandwich and a glass of wine is better than a hearty meal. A good dinner implies leisure for digestion. Half an hour's leisure before dinner will often enable a man to eat a hearty meal. Everything that is good is wholesome, taken at proper times and in proper quantities. After a man is twenty-five or thirty, he only wants as much food as will maintain his weight and not add to it.

Like St. Paul, he believes in a little wine, taken diluted at meals (provided it be good wine)—some good people will resent this advice—and thinks that spirits are useful when one is jaded and exhausted, but are unnecessary and hurtful when taken between meals or at bedtime, except for special reasons. "Three hundred and sixty-five glasses of whiskey taken in one year at bedtime are an unnecessary and a severe tax on the liver when its work is in full swing."

With regard to baths, an ordinary healthy man may take a cold bath daily almost up to any age; but as the object is not only to get up a reaction but to keep it, most hardworking men require that cheapest of all luxuries, a fire in the dressing-room and a hot bath-towel. If this is followed by a course of Indian clubs in his flannels, a man will be fit to face any weather. The same underclothing should be worn summer and winter, of wool, and only the outer clothing varied. This advice is better suited to England than to New England. Colds are generally caught either in ill-warmed rooms or through ill-protected feet. "If chilled through by a cold drive, walk home, if possible, the last mile or two, keeping on your heavy wraps to restore your circulation. . . . . Light your fire whenever you can endure it; it is the cheapest and best health given in the world, especially in cold, thundery weather in the summer; with a well-arranged room and a proper fireplace most healthy people can learn to sleep with their windows open, winter and summer."

Dr. Teale cautions against badly aired beds, and says "the risk will be best guarded against by carrying in the traveling-bag a light flannel dressing-gown to put on the damp sheets."

With regard to turning out of bed at night, no amount of precaution can make this otherwise than dangerous. But if an arrangement can be made by which the clothes can be kept aired and warm, and a cup of hot milk with a teaspoonful of brandy can be procured, the risks can be reduced to a minimum.

There is a timely word about holidays. Every medical man, if possible, should have an outdoor sport of some kind; golf and cycling are good, but perhaps the best is fly fishing. It takes one usually into a beautiful country, the exercise is gentle and varied, the interest absorbing, and it is better for the jaded practitioner than scampering half over Europe in a hurry in a second-class railway carriage in charge of a party of tourists.

Much of the above is what judicious medical advisers are constantly telling their patients; but it is not amiss that somebody should tell it to the medical advisers themselves. The doctor is apt to have less attention paid him, whether sick or well, than any one else.—Editorial, Boston Medical and Surgical Journal.

FORMALDEHYDE AS A DISINFECTANT.

The use of formaldehyde, or formalin, as it is trade marked, for purposes of disinfection, is growing in favor. This is due to the fact that the gas is at once very noxious to bacteria, and very innoxious to living animal tissues. It is stated by the highest German authorities that formaldehyde unites chemically with sulphuretted or nitrogenous products of fermentation or decomposition, forming new substances that are odorless, and in many cases antiseptic, thus destroying the food of bacteria as well as the bacteria themselves. Yet it does not discolor furniture or tapestries, and in moderate quantities is not harmful to the membranes of the throat and lungs of those who breathe it.

Baeyer believes that formaldehyde is the natural protector of the tender sprouts put forth by growing plants, being generated by the action of sunlight on the carbonic acid and water of the air in the presence of the green coloring matter of leaves, according to the equation—

$$CO_2+H_2O=CH_2O+2O.$$

Subsequently it becomes the parent of the carbohydrate structure of the plant. It is from this property of combining chemically with albumenoid or nitrogenous bodies that formaldehyde derives its germicidal and bactericidal power, since bacteria and micro-organisms generally are not only albumenoid in character, but their food is mainly albumenoid.

The distinfecting properties of formaldehyde were first set before the public in 1890 by Berlioz and Trillat, and there are already several forms of apparatus for preparing it. One of the simplest and most convenient of these is the Moffatt generator, made by Eli Lilly & Co., of Indianapolis. In form it suggests the ordinary Bunsen burner. It is designated to burn wood or other alcohols in a limited amount of oxygen:

$$C_2H_6O+2O=2CH_2O+H_2O.$$

It is claimed for this apparatus that a room may be thoroughly disinfected with it, without harm to the inmates of the room.—Pharmaceutical Era.

NEW METHOD OF PREPARING ANATOMICAL SPECIMENS.

Dr. N. Melnikoff-Rasvedenkoff.—The method of preparing anatomical specimens to which I wish to call attention comprises three different procedures, the results of which have proved excellent, for my first preparations, though dating from eighteen months, have not as yet changed their color in the least. These procedures are the following:

(1) The fresh organs are subjected, during from twenty-four to forty-eight hours, to the action of a 10 per cent. aqueous solution of formalin (a 40 per cent. solution of formaldehyde), to which has been added from 5 to 10 per cent. of sulphuretted hydrogen, or from 1-2 to 1 per cent. of hydrogen dioxide, and then laid for several days in 60 or 80 per cent. alcohol, after which they are put into a liquid of the following composition: distilled water, 100 parts; glycerine, 20 parts; potassium acetate, 15 parts.

(2) Excellent preparations are also obtained by first treating the organs with a 10 per cent. formalin solution, to which have been added small quantities of such substances as hydroquinone, hydroxylamine, pyrocatechin, glycerin and potassium chlorate, the subsequent treatment by alcohol, etc., being the same as above·

(3) Very good preparations are made by adding to the 10 per cent. formalin solution the following salts in the proportion of 3 or 4 per cent., viz.: the acetates of sodium, potassium, aluminium, ammonium, calcium, barium, magnesium, strontium, nickel and manganese, after which the organs are treated in the manner already stated by alcohol, etc.

The color of the fresh organs is thus fixed, not only by the formalin itself, but also by its solutions. To subject specimens to the action of one of these solutions, they are placed in a glass jar on a layer of cotton-wool.

The solutions used in the third procedure give very bright and clear colors, whereas with the second procedure they are quite dark. It is preferable, therefore, to combine the two solutions, the following combination being particularly suitable for the purpose: potassium acetate, 6 parts; potassium chlorate, 1 part; formalin, 20 parts; water, 200 parts. For preservation of the specimens, a solution of 20 parts of glycerin, 15 parts of potassium acetate, and 100 parts of distilled water is used. The specimens may be placed in glass jars with parallel sides, or fixed in gelatin in glass cases or in crystallizing vessels.—The Medical Week.

SOME LUSHAI PROVERBS.

Here are a few proverbs from Lushai-land: "A fly finds out where the sore is: a husband knows too much of his wife." "Well-bred people are always polite: only those of low birth can afford to be rude." "If you love your spouse, neither tell her your secrets nor make friends with young policemen." "Caulk a new boat; beat a new wife." "A woman's fortune is her face: a man's is his understanding." "If a man runs after a woman he falls into marriage; if a woman runs after a man she falls into ruin." "In jungle scrub the castor-oil plant is king." "Any knife is sharp enough to cut a potato." "A lost fish is always a big one: a lost child is always beautiful."—Indian Review.

Dr. Theo. W. Peers, of Topeka, Kas., says: I desire to report two cases in which I used Papine with very gratifying results. The first case was that of a man suffering with a non-operable case of epithelioma of the left side of the face. He had been operated on by a surgeon here, but on recurrence of the disease went to a "cancer doctor," who used a paste which "burnt" out a large amount of tissue, and started up a very rapid growth of the tumor.

When he came into my hands, in October, 1895, the disease was so extensive that to make him comfortable was all I could hope for. Morphine, cocaine and codein were tried, but with such distressing after-effects that they had to be abandoned. I then began using Papine, and two to four doses a day of a teaspoonful each kept him comfortable, with absolutely no unpleasant after-effects and with no increase in the amount given per day. The rapidity of the growth was decreased so that he lived until June, 1896, whereas, when I first saw him I did not think he could live three months.

The other case was one of probable tubercular peritonitis. I used it for six months with no after-effects, and always with relief to the patient. I know of no other anodyne that could be used for so long a time without unpleasant after-effects and without increasing the dose.

THE INTERNATIONAL MEDICAL CONGRESS.—The following are the names of those who have been invited to deliver orations at the general sessions of the Moscow Congress. The titles of all the orations have not yet been announced. Lauder Brunton, of London, on the "Relationship Between Physiology, Pharmacology, Pathology and Clinical Medicine;" Nicholas Senn, of Chicago, on the "Diagnosis and Surgical Treatment of Acute Forms of Peritonitis:" Krafft-Ebing, of Vienna, on the "Etiology of Progressive Paralysis;" Danilevski, of Khartov, on "The Action of Electric Rays on the Animal Organism at a Distance;" Lombroso, of Turin, on

"New Horizons in Psychiatry;" Leyden, of Berlin, on the "Present Modes of Treating Consumptives and their State Control;" Herzen, of Lausanne, on the "Significance of Physiological Psychology for Medical Education;" Robert, of Barcelona, and Lukianov, of St. Petersburg, have not yet announced the titles of their addresses. Virchow, of Berlin, and Roux, of Paris, have been invited to deliver orations, but no word had been received from them by the Moscow committee at the time the above announcement was made.

Information of service to those intending to attend the congress is very difficult to obtain. Dr. A. Jacobi, chairman of the American National Committee, which was established at the request and under the authority of the general committee of the Twelfth International Medical Congress, writes to inform the numerous gentlemen who are constantly applying for information concerning certificates, trip, fares, hotels, etc., etc., that he has none to give, not having heard from the general committee these two months.

Yellow Fever From the Isthmus.—Cases of yellow fever are being brought from Panama to both the Atlantic and the Pacific ports of the country. Nearly every ship arriving from Colon now has one or more deaths among the passengers, and the City of Para arrived in San Francisco on June 13 with one case of yellow fever aboard, two deaths having occurred during the voyage from the Isthmus. It is stated that the disease is epidemic at Panama on the Pacific side, but that no cases have yet occurred at Colon on the Atlantic side, all the passengers to this port who have had the disease having contracted it at Panama. No apprehension is felt of the entrance of yellow fever into New York.

The Accommodations at the Moscow Congress promise to be somewhat limited. The reception committee has been able to find but seven hundred beds upon which they can count in the hotels, and sixteen hundred in furnished apartments scattered throughout the city. In addition, about fifty Moscow physicians have announced their readiness to entertain each one or more guests during the Congress week. A circular containing information as to rooms and charges will be handed each member upon his arrival in Moscow.—Medical Record.

THE INFLUENCE OF LIGHT ON THE SKIN.

In the July number of the British Journal of Dermatology Dr. Robert L. Bowles states that he has for many years been engaged in investigating the effects of sunlight on the human body, and especially the penetrating effects of rays reflected from snow and other surfaces. Professor Röntgen, he says, finds that certain rays generated or excited by electrical action penetrate most of the human tissues and other substances, and are stopped by substances of a different nature. The author, on the other hand, has demonstrated that reflected luminous or photo-chemical rays also penetrate the human skin into the deeper tissues beneath and produce within them great and important changes. He gives the following summary of facts and conclusions of various phenomena observed by him from time to time:

1. That heat *qua* heat is not the cause of sunburn.

2. That there is strong reason for believing that sunburn is caused by the violet rays or ultra-violet rays of light reflected from snow, and that it is not necessarily of the same quality as that which is incident.

3. That Captain Abney finds that the violet or ultra-violet rays are very strong at high altitudes, and believes that altitude has much to do with sunburn.

4. That altitude alone does not explain sunburn, for one may be unburned on rocks, say at ten thousand feet, and yet become immediately affected on descending to a glacier three or four thousand feet lower down.

5. That sunburn and snow-blindness arise from similar causes, and that sunstroke and sun fever may be associated with the effects of penetrating light rays.

6. That rays from the electric light produce much the same results as sun rays reflected from snow.

7. That the bronzing of the skin and the browning of the wooden châlets are probably produced by rays reflected from snow.

8. That various pigments, but chiefly those containing red and yellow, stop or alter reflected rays and prevent the physiological and pathological changes usually due to them.

9. That freckles, which are but the milder effects of luminous or chemical rays, stop the penetration of those rays through the skin.

10. That the sometimes very serious inflammatory changes in sunburn and in what Mr. Hutchinson designates "summer eruptions" are due to the penetration of reflected luminous or photo-chemical rays through the skin to the deeper tissues beneath.

11. That photography often demonstrates the existence of freckles and, report says, various eruptions deep in the skin which are perfectly invisible to the naked eye, showing that the luminous or photo-chemical rays are stopped or altered by them, and not reflected back, as no change is produced on the negative—an effect which suggests that these photogenic rays have penetrating powers as yet unknown.

12. That the wood of Swiss châlets is burned perfectly black (carbonized) on its surface by rays reflected from snow, which rays in time penetrate deep into the substance of the wood and change it to a dark-brown color.

13. That the first effect of snow rays on a new châlet is shown by its action on the resin of the wood, which "sweats out" and leads more easily to the charring of the woody fiber itself and the subsequent changes in the deeper parts.

14. That Captain Maude, R.E., has shown from his own personal experiences and from experiments on many friends, that solar rays in India produced sun fever of a very serious kind, which was entirely prevented by the wearing of an orange lining to all his clothes and inside his hat. These experiments demonstrate the penetrating power of light rays through clothes unprotected by color, and their important influence on health. In relation to this, the author has shown that a lady wearing a linen blouse with red and white stripes was strongly marked with red and white stripes on her shoulders, but the red line on her skin corresponded with the white lines of the linen—that is, the red stripes had completely stopped all rays from affecting the skin beneath them.

15. That the author has often shown that rays reflected from certain surfaces—such as water, gold and silver lace, white walls, white veils, certain clouds and mists—act physiologically in a peculiar manner and quite differently to direct light, and that some physical change hitherto unexplained must take place in light during or after reflection.

16. That in relation with the foregoing are those marvelous changes in the vegetable kingdom connected with the formation of chlorophyll and the deposition of starch.

From these and many other observations Dr. Bowles says that he cannot help feeling that Röntgen rays may be modifications only of ordinary light, and that their further elucidation must go hand in hand with a further inquiry into the profound changes caused by reflection to which he has above referred. It need not necessarily be assumed, he thinks, that what we call darkness implies an absence of all the forms of light.

In August, 1896, the author, assisted by Mr. Travers, undertook some experiments to discover the cause of the radiant energy from snow being so much more irritating to the eyes and skin than direct energy from the sun, as follows:

1. To show the relative values of sunlight and snowlight in freeing iodine from its combination with hydrogen.

2. To test the relative effects of sun on xxx Paget's plates, inclosed in cases made of aluminum and cardboard, in producing pictures like those of Röntgen.

These exposures, says Dr. Bowles, were effected on snow at an elevation of eight thousand and forty feet, near the hut of the Ober Aletch Glacier, on three consecutive days. The weather was bad and uncertain, but there was some sunlight, and some interesting and definite results were obtained.

Bottles containing equivalent proportions of a sulphuric-acid solution and potassium iodide were fixed in cases and exposed simultaneously to the sun and snow; some bottles were coated with pigments, others with cloth of various colors, but each case contained also some of the solution in an uncovered bottle as a control experiment.

The results were expressed in iodine equivalents, and they appear, he says, as Mr. Travers expresses it, "to indicate that the actinic value of the reflected light from snow is somewhere about 0.7 to 0.8 of the value of the direct rays of the sun."

The x-ray plate exposed to the sun displayed skiagraphs of a piece of tin, whereas on the plate exposed to the snow no change could be detected; but as there are reasons for the possibility of this plate having been spoiled, this experiment was not conclusive. The author quotes Mr. Travers, in his remarks on the experiments, as follows:

"In dealing with the published accounts of the cases of sunburn and dermatitis produced by the so-called x-rays, it is not at all certain that the injury done to the hands and arms of the operators was due to the rays which are capable of penetrating aluminum sheets, etc. We know that the radiations from a Crookes's tube include rays which come within the visible portion of the spectrum, and it is to these rays that we may attribute the power of producing sunburn.

"Further, while it takes a very long time to produce sunburn in the neighborhood of a tube which will fog a photographic plate, contained in a dark slide, in a few seconds, light reflected from the snow will sunburn in a very short time, but will not fog a plate in a dark slide.

"In a paper which appeared either in the Lancet or British Medical Journal a few weeks ago, it was shown that the effect produced by certain kinds of light—e. g., light from incandescent gas or arc lamps—produced injurious effects. The injury could not be attributed to the presence of a greater intensity of ultra-violet or violet light than was present in sunlight, but was due to the absence of 'red' radiation.

"In the reflected light from snow the heat rays are nearly entirely absent, the violet (chemical) rays would be present with almost the same intensity as in the direct sunlight.

"Considering the lack of evidence in favor of the x-rays pure and simple being a cause of dermatitis similar to sunburn, it is worth while reviewing the facts in support of the theory that the true cause is to be found in the violet or chemical rays, or in the increase of intensity of the violet rays with regard to the intensity of the red rays.

"I think that the facts cited in your paper (British Medical Journal) March 7, 1896, furnish sufficient support."

Here, continues the author, from an entirely physical point of view, Mr. Travers, like others, arrives at conclusions similar to his own—namely, that the vital changes on the skin are due to the chemical rays, and apparently to those rays alone, and that the rays issuing from a Crookes's tube are not an entirely new form of energy distinct and separate from light, but contain a proportion of luminous and chemical rays, and that light, as such, as well as the divisions into which it can be split up, may penetrate wood, clothing, and the human tissues.

Dr. Bowles refers to Dr. Unna, who, in 1885, dealt very fully with the subject of pigmentary changes in the cutis, and suggested that they depended on the effects of the chemical rays, and that curcuma and colors acting on the light rays would prevent changes taking place. He refers also to an excellent paper by a Dr. Hammer, of Stuttgart, entitled "The Influence of Light on the Skin," in which the author deals with the subject first biologically—for example, the action of light on worms and other sightless creatures influenced by light through the skin. On the physical side he quotes the experiments of Terrier, Malakoff, and Widmark. From the beginning to the end, Dr. Bowles says, he found Dr. Hammer's work entirely corroborative of his own, but thinks that Dr. Hammer is not sufficiently impressed with the important fact that reflected light burns much more rapidly than direct.—N. Y. Medical Journal.

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REVIEWS.

Surgery of the Rectum and Pelvis. By Charles B. Kelsey, A.M., M.D., Professor of Surgery of the New York Post Graduate Medical School and Hospital, etc., etc. With 281 illustrations and half-tone plates. New York, Richard Kettles & Co., No. 129 Fifth avenue.

The work of Dr. Kelsey on diseases of the rectum has long been one of the standard books in the English language on the subject, and the fact that he has added the pelvis to this title, indicates the increased scope which a growing experience of twenty years has seemed to compel in a proper consideration of the subject. He says, "the impossibility of separating diseases of the rectum in practice from so-called gynæcology and genito-urinary diseases, is apparent in such cases as laceration of the sphincter, proctocele, recto-vaginal fistula, pelvic abscesses in women, displacement of the uterus, acute and chronic prostatitis and hypertrophy of the prostate, all of which are constantly associated with rectal symptoms." Whether the profession in general will agree with the author in thinking that he has improved his special work by including parts of another specialty, may be open to some question. Indeed, if he is going to consider everything that is disarranged by rectal disease or all conditions associated with rectal symptoms, he will certainly be carried very far beyond diseases of the rectum. The whole intestinal tract from mouth to anus, the liver and kidney, and, indeed, the whole system, would have to be also considered; but whenever this author writes and on whatever he writes, we find material worth reading and illustrations which are of practical value; and so it is with the articles we find here in regard to vaginal operations and hernia operations. That he has added anything of especial value to the subject beyond his immediate specialty we are unable to discover from an examination of his work, unless perhaps his illustrations of hernia operations, and his exceedingly lucid statements in regard to vaginal operations, together with the illustrations, would enable some practitioners to more readily understand the steps which have long been followed by those who have made a specialty of this work. Of course, the volume must bring into discussion the limitations of a specialty, and if our readers will recall some remarks in a previous issue of the JOURNAL, in regard to specialists and specialism, they will probably see that we were not far from the facts. We still believe, however, that the rectal specialist who includes gynæcology, will be very much less likely to have rectal cases referred to him by gynæcologists, and so for the specialist in any other line who takes in more than the specialty for which his name stands as a synonym. As a matter of fact, a good general surgeon ought to be able to do any rectal, vaginal or abdominal work, as well as the men who pretend to do only such work. But this is taking us out of the way, and we return to the volume to say in regard to it, that it still, as a work on rectal diseases, must command the attention and support of the surgical world.

Reference Book of Practical Therapeutics. By various authors. Edited by Frank P. Foster, M.D., Editor of the New York Medical Journal and of Foster's Encyclopedic Medical Dictionary. In two volumes; volume 2. New York, D. Appleton & Co., 1897.

In regard to the scope and general character of the work, we had occasion to speak at some length in a notice of the first volume that appeared; and we are glad to be able to confirm what we then said as to the thoroughness and completeness with which the subject of therapeutics is handled in these two volumes. The second volume consists of nearly 500 pages of text, with a complete index of the whole work, making a total of more than 600 pages. The subjects are considered in the alphabetical order, and this volume begins with napelline, the new drugs, and the new methods of therapeutics have been given, and the balance of the various articles seems to have been entirely preserved. To illustrate, in the first twenty-five pages we find a considerable portion of the space taken up with a consideration of nitric acid, nitrites, nitro-glycerine, nitrous oxide and nucleins, and yet in this same space there are naturally a great many other things touched upon. The manner in which the subject is treated is no better illustrated than by the article on opium, which, while it does not pretend to go into all of the details concerning the countless therapeutic applications of the drug, does consider many cases in which it is of especial value and the cases in which it is contra-indicated, giving also a good resume of the toxicology of chronic opium poisoning and its treatment. We do not believe that the author of the article gives sufficient importance to the use of potassium permanganate in the treatment of opium poisoning when he dismisses the subject in a few lines; nor do we see anything in regard to the use of hydrochloric acid. On the other hand, perhaps because it is so well known and so extensively used as an antidote, atropine is carefully considered in this capacity and a wholesome warning given against killing the patient by the combination of both drugs. We believe that such a thing has occurred more than once. Again, the question of serum treatment, which is to-day occupying so much attention, takes up fifteen pages of the work. Whether or not serum therapy has come to stay time must determine, but it certainly fills at the present time a role not less important than the author and edior of the volume have accorded it in this article.

Another subject which has been before the profession during the last few years, is presented with the candor and thoroughness characteristic of the author. We mean toxines, especially the toxines which have been used in the treatment of malignant growths. As no table shows that the results have been most favorable in the spindle-celled sarcoma, but as we contended at the time of the first report in regard to spindle-celled sarcoma, if there were a single case of either sarcoma or carcinoma which had been abandoned as intractable by operation or otherwise, and which yielded to no form of treatment, that form of treatment must command the respectful attention. The author gives the condemnatory views of the committee appointed by the New York Surgical Society to investigate and report upon erysipelas toxine treatment of malignant growths, and adds that their views are properly conservative and in accord with the spirit in which Dr. Coley's experiments have been carried on. Considerable space is given to mineral waters and a supplement of about fifty pages completes the work. We do not hesitate to say that it is a most useful addition to the library of any practical medical man.

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EDITORIALS.

OBITUARY.

William T. Lusk, M.D., LL.D.

It will be a source of great grief to many of our subscribers, as it has been to his collaborators on this journal, to hear of the sudden death of this eminent and valuable medical man. William Thompson Lusk was born in Norwich, Conn., in 1839, and was pursuing his medical studies at the outbreak of the war. He enlisted in a regiment of New York Volunteers and rapidly rose to the rank of Adjutant-General. Leaving the army in 1863, he resumed his medical studies and graduated from Bellevue Hospital Medical College in 1864. The next four years found him in professional study abroad, at Paris, Edinburgh, Vienna, Berlin, Heidelberg, and other medical centers. He received the degree of Master of Arts in 1872, and that of LL.D. in 1893 from Yale University. He was for a time a lecturer on physiology at Harvard, and afterwards the Professor of Physiology in the Long Island College Hospital, Brooklyn. He was for many years the Professor of Obstetrics at the college from which he graduated, and his work on this subject is probably better known than that of any other writer in this country or abroad. He was a strong personal friend of the founder of this journal, and had been for many years one of its collaborators. Modest, unassuming, and with it all a man of high scientific attainments, he will be missed by the whole profession perhaps as much as any man who has lived in our day and time. He was at the zenith of his intellectual and professional life, and his sudden death on the 12th of June from cerebral apoplexy was perhaps as great a surprise as could have occurred in the ranks of medical men. We cannot say that the good which he has done is interred with his bones, for apart from the many medical essays of value which he has contributed to the profession, and apart from the practical work which he has done in the saving of life, his volume on obstetrics will remain a monument to his patience, skill and intellect.

THE QUESTION OF REVIEWS.

Reviews and reviewers are by no means a new theme for consideration, but it does seem to us that there is a great deal to be said in behalf of the reviewers, especially those who have to do considerable work in this line. It is true of them, as it is of other workers, that as their experience increases their work ought to be of more value, not only to the publishers and authors, but to the journals in which they appear; and yet we do not believe that we are misstating facts when we say that the more reviews a man writes the harder it is for him to write one that suits either himself or the public. In the first place, he is confronted with the fact that he has before him the result of probably a life's work of earnest, honest effort of an able man, of a man much more capable of expressing opinions on the subject of which he writes than many others, including, perhaps, even the reviewer himself. He is aware of the fact that whatever he may say will bear fruit of some kind. He is aware that there are already a great many volumes, perhaps, on the same subject, many of which are masterpieces, and he must keep this in mind in giving a statement in regard to the book under consideration. Practically, all of the journals in the world have long ago settled the question as to the personality of the reviewer by always giving the review without any signature, and this is as it should be; but this does not relieve the reviewer himself of the responsibility entailed by his work. We have said that he has first of all the author to consider; the years spent in the preparation of the work. There are others, too, who must receive no small share of attention, and they are the public for whom the work is intended and the publisher by whom the work is done. It is, perhaps, better to couple the publisher and author, as so often the work of a life is marred by its unfortunate presentation, and, on the other hand, a work of no special merit may receive quite an abundant patronage, owing to the source from which it is published and distributed. It may be said, however, with all candor and fairness, that the numerous good medical publishers of this country all do their work with a thoroughness, uniformity and excellence which challenges comparison with the best work of any other country, and that the authors have very little to fear on this score. But how about the public? We mean the medical public, because we presume that medical books that possess any scientific value are meant only for medical men. If this be true, the patronage is at best limited; and yet if the work is such as to merit the support of the profession, if its support be at all general, it will result in a good return pecuniarily to both the publisher and author. But it is a well known fact that medical men very seldom, if ever, attain any considerable income from their medical works, while the publishers have grown rich. The incentive, then, to the medical man to give his days and nights of toil and labor outside of the ordinary and difficult routine of his practice to the composition of a work of value, must be something more than the possibilities of, at best, a small pecuniary compensation. This, then, simply increases for the reviewer the responsibilities of his task, in view of the fact that the author has undertaken in many instances a work for the enlightenment of his fellow-workers as a work which can, per se, bring him practically never anything like an adequate return for the labor expended. The medical public, on the other hand, is probably already loaded to the guards with works on the same subject; and while many are waiting to get a good work and a new one, they are unable to judge for themselves of the merits of such a work, and must be guided entirely by the reviews which they find in their medical journals; and of these medical men there are too many, we are sorry to say, who can ill afford to make any experiments in the line of books. To the country practitioner the medical work is a bible; it is read with diligence and is followed with perseverance and persistence; and if dangerous doctrines are contained, not only does the general public suffer, but the physician who follows such doctrines suffers in more ways than in reputation. We come back, then, to the duty which devolves upon the reviewer. He must tell what he feels to be the truth. He must do this, of course, in an entirely impersonal way. He must write of the book as it appears to him. It would be simply impossible to take up and consider the many things which he would like to do in the course of his work, and yet he must consider something, he must say something, he must do what he feels to be fair and right. If there be a *ratio esse* for the existence of the book, he must see it and acknowledge it. If there be not, he must call attention to it. If there be doctrines that are dangerous and unwholesome, he must make mention of them. Every one knows how easy it is to find fault and how hard it is to commend. The consequence is that frequently the best reviewers of even an important work will occupy a comparatively small space, because there will be, perhaps, little if anything to commend, and not much more to select for special condemnation. If, then, the reviewer gives a clear statement of the facts—the causes which call the book into existence, the scope and character of the work itself, with special praise or reproach as occasion demands—and endeavors to do this in all cases alike, neither the public nor the author ought to have reason to complain.

AN UNFORTUNATE MISCARRIAGE.

A number of our contemporaries were filled with plaudits of a scheme for the increase in the advantages to medical education in this city by the consolidation of two of the well known colleges. Tne unselfishness of the professors in both, the breadth of comprehension of the directors in each, the earnest desire on the part of all to improve medical teaching in general and the instruction in this city in particular, led us to believe that we were approaching the millenium. The exceedingly novel plan adopted for the selection of the faculty of the combined college, seemed to preclude any possibility of a failure of the scheme. The plan was for each professor to nominate a man for every other position in the combined college except his own, and for his own chair to nominate his second choice, the presumption being that he would choose himself above all others. Strangely enough, no satisfactory agreement could be reached, and, unfortunately for the consummation of the deal, one or two of the very able men who were especially interested in the matter were carried off by the Great Destroyer about this time. Barring this, it is not so unlikely that some sort of an arrangement would have been reached. The present outlook, however, is that the two colleges will continue to carry on their work as heretofore, independently and separately, and that the immense advantages which were to accrue to the coming professional men are at least delayed, if not entirely lost. Really, it is very much to be regretted that such a consolidation did not take place, and if some of our Western cities would follow the idea and reach a satisfactory conclusion and consolidation, we believe that their sphere of usefulness would be considerably increased.

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ORIGINAL ARTICLES.

DISCUSSION ON THE USE OF THE PESSARY, ETC.

The Curette in the Treatment of Displacements.

By ARTHUR W. JOHNSTONE, M.D.

Mr. President: I want to apologize to this society for not being ready with a written paper; I am ready without it, however. My part was to be on "The Curette in the Treatment of Displacements." I will say I have seen about a dozen cases cured absolutely by the curette and nothing else. They were largely in young girls, where there had been a catarrhal inflammation going on for some time; where the inflammation, the blood stasis and all, had so extended to the general pelvis, and the round ligaments and the broad ligaments themselves were so soft that a full bladder or anything of that kind would cause the uterus to be thrown out of position.

The first case I saw was mistaken for a fibroid, but I found it to be only a displacement of the uterus. The uterus was simply an old pus pocket, that needed cleaning out like any other retention pocket, and there was, in addition in this case, a peritonitis. To my surprise and great pleasure, within two months after curetting, without a pessary being used, that uterus backed itself around to its normal position. The inflammation was absolutely cured, and the only way I could account for it was that by getting rid of this œdema the natural supports of the uterus had shortened up and pulled the uterus into position. Since then I have been watching cases of curetting, and I have come to look on versions as very largely complications of old cases of metritis. It is true, I know, the unusual Alexander operations have shown that some cases never have the round ligament properly placed. Like the undescended testicle, the end of the round ligament seems to wander up along the inside of the abdominal wall and does not get its proper attachments and does not take up its slack. There may be a few cases of

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*Read before the Cincinnati Obstetrical Society, April 8, 1897.

congenital retroposition. And then I have seen a few cases that I am sure were caused by trauma. In one case a young woman was thrown off a horse, and after turning a double somersault lit on her feet and suffered afterward with retroversion. Another one was a girl who rode a bucking horse and was taken off the horse with a retroversion, and it remained until afterward, when she was curetted. She, by the way, also had a chronic metritis. I tell you of these cases to give you my ideas of what versions really are. The organ, by chronic inflammation, has gotten its supports weakened, and then by any accident like these the version may be caused, and if it is thrown back and held there long enough these ligaments will be permanently lengthened. I have looked up the statistics, and I suppose about 20 per cent.—not more than that, taking the cases as they come—of these old versions with chronic metritis are cured by the curette and holding the uterus in its proper position for one or two or three years with a pessary.

There are a great many others who are made perfectly comfortable as long as the pessary is worn and who are perfectly satisfied to get along with it. Unless there has been some pelvic inflammation or some firm adhesions on which the uterus rides I believe the patients could wear a pessary for some time. But you must be very careful. I remember a lady from across the waters who had some version, and a French surgeon had placed a pessary, and when she came to me it was a question whether her ovaries should not come out. There was a decided swelling on the right of the uterus, probably of the ovary or tube. I found above the insertion of the uterus, in the posterior part of the vagina, a lump which I took for a fibroid. On inquiry she told me she had had a cyst taken out, and this pessary was placed so as to rest around the scar of the cyst. I took the pessary out and let her go for a few weeks. You would have been surprised to have seen how quickly and rapidly all the bogginess about the pelvis disappeared and how quickly and promptly the case got almost well, showing no pessary was required.

There was one case in which the pessary did a great deal of harm. Many of these cases have bad appendages, and it is all right to curette these cases, but bad practice to put in a pessary, particularly if the pessary will rest on the appendages in any way. So that in that class of cases, where there has been more or less occlusion of the tube or adhesions, or where the fire has already rushed through, the pessary should be kept out. It is only in the class of cases where the uterus comes up freely and there is no ovary or tube that the pessary can possibly rest upon that I would ever advise it to be used. But, unfortunately, gentlemen, quite a large number of cases will have complications, while if the pessary had been used at the right time these might have been avoided, but it has gone on beyond that until the patient has had pelvic attacks which have glued everything together. What is to be done there depends on the circumstances, financially, of the patient and the amount of trouble given. Take, for instance, a wealthy woman with adhesions and a retroversion with a chronic metritis—I have several on hand now. You curette the uterus and get rid of the original source of trouble; even though there are adhesions there those ladies will be able to attend to the social duties and take long trips to Europe and here and there and everywhere. They will have attacks now and then, but most of them you will find will refuse any kind of operative procedure. And you must remember that this thing of breaking up the adhesions, while it looks very pretty on paper, still it is about the same as the operation for taking out the appendages, and unless it is a threat to the woman's life or to her usefulness as a citizen, I think we had better let well enough alone.

Where the uterus is easily put up, I put in a pessary. Where the adhesions are not very great and do not give much pain, if the patient is up and about and able to go back to her position as a citizen and all that, I think we are wrong to urge an operation; but, if there has been an accumulation in the tubes or the ovaries, or if the adhesions give a great deal of pain, then it is time to think of operation for correcting the position of the uterus. But in that class of cases I think we operate more for the appendages than we do for the uterus itself.

Now for the mechanical means of fixing the uterus. The one that has appealed to me and the one I like best of all is the Alexander operation. It is simply carrying out nature's method, assisting nature by shortening her natural support. I am glad to say I saw the first case I ever did two weeks ago. It was done three years ago. The woman is very comfortable, has gained in weight, and the uterus is in its proper position. The girl in that case was very uncomfortable before the operation, and it has proven to my judgment that it is the one of all others I would want to do. As for the fixation of the uterus to the abdominal wall, I did not hear the previous discussion, and, of course, will have to go on what my studies have been. I have nothing for this but condemnation; I never liked it, and I see the more experience the world in general is having with it the less they are having to do with it. Like every other fad, it has run its course and is dying out.

Now as for the method of doing the Alexander operation. This operation is not a hard one, but comparatively easy. Particularly I like the idea of opening the lower end of the canal itself. Even though there was in that case a large sheath of peritoneum pulled down, still it is, compared with laparotomy, mere child's play. I don't know but

Martin's idea of taking both and tying them under the skin is about as sensible as any. Then you do not leave buried ligatures and run less langer of sepsis.

As for vaginal fixation, I do not like it, and if pregnancy goes on we may have diastrous results. There is another way of shortening the round ligaments, spoken of by Polk, of New York, which has struck me as reasonable because it gives an opportunity for breaking up the adhesions, which the Alexander operation does not. That is to go down between the uterus and the bladder and stitch the round ligaments together. In the two cases in which this was done the operator attempted to bring the uterus forward and fasten it. Whether it was an irritating catgut ligature or what I do not know, but I know I had to make her many visits for a boggy, thick, hard, indurated swelling that seemed to be general. It left the whole pelvic condition worse than he found it, and if the patient had not been at or past the change of life we would have had considerable trouble, for there surely would have been an abscess.

So, Mr. President, as I find it in my private work, having no hospital appointment and not seeing the laboring classes so much, my experience is that these operative fixations of the uterus are comparatively rarely needed. It is not often they are required. In the working classes you do need them a good deal probably, but in my experience it is rare you have to operate for a malposition, and that you can correct it in a much simpler way than by doing any of these operations, unless it be the Alexander operation, which is a comparatively easy affair. But when ve do a laparotomy we may fasten the uterus forward, and the way that is best done, I believe, is just to get in behind the round ligament and double it up in a knuckle and leave it there. But that does not act in all cases. But the shrinkage of the fibers is often satisfactory.

HYSTERIA IN EARLY LIFE.*

By AUGUSTUS A. ESHNER, M.D., Philadelphia,

Professor of Clinical Medicine in the Philadelphia Polyclinic; Physician to the Philadelphia Hospital, etc.

Through the labors of the French school of neurologists in particular, hysteria has been given the recognition it deserves as a distinct, fixed clinical entity, with a train of symptoms as characteristic as those of any of the acute specific infections or intoxications. That the disorder has a pathology of its own I have no doubt the results of future investigation will demonstrate, but as yet we need more knowledge, especially in the domain of physiologic and pathologic chemistry, before we may hope for a solution of this aspect of the problem. From both inference and analogy it seems not unreasonable to believe that hysteria depends essentially upon metabolic or nutritional changes in the cellular elements of the central nervous system, in consequence of which there may result alterations in function and changes in relation, whence arise the varied and protean symptoms of the developed disease. It may not be extravagant to hope that further refinement in staining methods, in which there has been so remarkable an advance in the past decade, may make possible the detection of changes in nerve-cells that at present elude closest scrutiny by existing means of investigation.

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* Read before the Philadelphia County Medical Society, June 23, 1897.

Though we retain the name, which perpetuates the original erroneous conception of its pathology, we have learned that hysteria may exist not only in women deprived of their uteri, but even in men as well. Hysteria respects neither sex nor age, although by far more common in females than in males, and comparatively rare in early and in late life. The explanation of these differences must be looked for in the varying susceptibility and receptivity of the nervous system with regard to those influences to which in a general way we have learned to attribute etiologic activity. According to statistics cited by Lloyd in "A Text-Book of Nervous Diseases" (edited by Dercum), hysteria is most common in women at the age of twenty years. Briquet found one-fifth of the cases in the female sex to occur before puberty; and rather more than one-third between the ages of fifteen and twenty. Batault found the disease most frequent in men between the ages of ten and twenty. Mills, in the "Cyclopædia of Diseases of Children" (edited by Keating) reports a case of catalepsy or automatism in a girl two years old, and refers to a similar case reported by Jacobi in a child three years old. He cites also a case of hysterical paralysis in a girl eighteen months old, reported by Gillette. The evidence goes to show that hysteria, while perhaps not so uncommon in childhood as it appears to be, is yet sufficiently so to warrant the report of a small group of cases illustrating some of the phases of the disease as it appears in early life, as well as some of the difficulties and doubts that at times attend its recognition. The causes, symptoms, course and treatment of hysteria are much the same in children as in adults, except in so far as these are influenced by modifications dependent upon differences in mental and physical growth and development.

Case I.—N. G., a schoolgirl, nine years old, presented herself in the clinical service of Dr. Morris J. Lewis, at the Orthopædic Hospital and Infirmary for Nervous Diseases, on April 21, 1897, with the statement that about a month previously she had been much frightened by being placed by her father, who was at the time intoxicated, in a room apart from the rest of the family. The child cried and complained of feeling sick. A few hours later, during the evening, while in bed but still awake, the right hand and arm began to tremble and soon the head likewise to shake. In the course of several hours more the left hand also began to tremble. There was no convulsive movement and no loss of consciousness. The little patient now fell asleep and slept quietly through the night. On arising the next morning the movements returned, and, besides, there occurred in the lower extremities rapid movements resembling those made by a horse in trotting.

In this attack it is said that consciousness was lost, the eyes being closed and the teeth being grit, but the tongue was not bitten. The attack lasted about five minutes, and at its close the child was quite itself again and in no wise dull. In the course of the succeeding day some ten or twelve attacks of like character took place, but sleep was undisturbed during the night.

On the following day the attacks were repeated with about the same frequency. In the next three weeks the number of attacks averaged fifteen a day, but after this it reached as high as thirty. In none had the tongue been bitten, but the mother maintained that consciousness was lost in all. Each attack was followed by headache. No attack was known to have occurred during sleep. For a week the gritting of teeth had ceased.

A number of attacks occurred at clinic and presented the following features: While sitting the child suddenly began to droop her head, then to move it forward and backward. Next the right hand began to tremble, and then the left. Finally the arms and legs also were set in active movement, as in the process of walking on all fours. Frothing at the mouth occurred. The eyes were closed and the child appeared unconscious. The attack terminated in clenching of the fists and tonic rigidity of the members. The mental state following was unobscured. In the intervals between attacks there was tremor of the right hand, which ceased with the onset of the attack, and also when the child's attention was diverted or engrossed, as in conversation.

The child was well nourished and of good color. Gait and station were normal; the grasp of the hands was puerile; the knee-jerks were capricious. The action of the heart was rhythmic, its sounds clear. There was no obvious derangement of tactile or painful sensibility. In the family history the only points worthy of note are the occurrence of chorea in a cousin and of rheumatism in the mother's family.

The patient herself had been born at term, had nursed at the breast for sixteen months and had been free from noteworthy illness and from convulsions during early infancy. She began to walk at the age of fifteen months, and was rather late in speaking. At the age of two she suffered from measles and at four from whooping-cough. Following the latter persistent internal strabismus had been noticed. Epistaxis had been frequent for four years, occurring mostly at night, but apparently being unattended with evil consequences. For a year the bleeding from the nose had been replaced by periodic headache, which was greatly relieved by the prescription of glasses correcting a high degree of hyperopia and astigmatism. There were no changes in the fields of vision. The child had suffered frequently from attacks of croup. She cried upon the slightest and even without any real provocation.

She was directed to take ten drops of peppermint water and the correction of the refractive error undertaken, and she was told that if she did not speedily improve she would be placed in the hospital. The attacks at once moderated in frequency and severity, and the general condition was substantially improved.

There can, I think, be no doubt as to the hysterical nature of this case. Though lacking in some of the details of the complete clinical picture of hysteria, it yet presents so many distinctive features that mistake seems scarcely possible. The mode of onset, the character of the symptoms, the paroxysmal seizures, the emotional mobility, and finally the results of treatment, constitute such a grouping as is not encountered in any other disease. The loss of consciousness, whether merely apparent or real, while of course suggestive of epilepsy, cannot be held to exclude hysteria, as such true loss may, I believe, attend the latter as well as the former condition.

CASE II.—N. B., a girl, nine years old, came to the clinical service of Dr. Lewis at the Orthopædic Hospital and Infirmary for Nervous Diseases on April 14, 1897, with a history of several times daily, for two years, going through a series of peculiar movements, which consisted essentially in dropping the arms and spreading out the hands as if to catch herself in the act of falling, without loss of consciousness. She had had, besides, three convulsive seizures, in which she kicked vigorously, rolled up her eyes, etc., and in which it was thought that consciousness was lost. In some of the attacks urine was passed involuntarily, but the anal sphincter was continent and competent. In none was the tongue bitten. The child was exceedingly emotional, crying readily, and being subject to attacks of causeless laughter. At times there was headache. The knee-jerks were preserved and station was steady. The dynamometric record was ten in each hand. There was no sensory derangement. The family history showed no evidence of neurotic predisposition. The little patient herself had never been seriously ill.

Under date of June 21,1897, it is noted that the onset of the attacks following the eating of a quart of peanuts. At that time the child had a continuous series of convulsions for two days. At present it is said that the seizures are repeated at intervals of five minutes during the day, although but one occurred during the quarter of an hour that the child was under observation, and in this, which lasted but a few seconds, the child doubled up on its mother's lap slightly and dropped its head forward. It might have fallen had it not been supported. The attacks take place not only in the presence of others, but also when the child is alone, and she has injured herself in several. They are superinduced by the ingestion of such articles of food as meat, cabbage, tea, coffee, etc.

The child is exceedingly pallid; its digestion is poor and its bowels constipated. Pin-prick is everywhere readily appreciated. The heart is said to beat rapidly at the close of the attacks, but auscultation fails to disclose evidence of organic disease. Dr. A. G. Thomson was unable to detect any abnormality of fundus or of muscular balance. Hypnosis was attempted, but apparently without success.

While some of the features of this case are strongly suggestive of hysteria, others are not less strongly suggestive of epilepsy, and without further study the discrimination is by no means easy. I admit that the criticism may be justly made that the doubt surrounding the diagnosis should be sufficient to exclude the case from this report, but if there is no other justification for its inclusion I may be permitted to retain it in order to emphasize the difficulty with which the differentiation of the two diseases is sometimes attended and to dwell briefly upon the fact that the same patient may be the victim of both.

This difficulty in diagnosis, and especially as it occurs in children, is illustrated by a case that came under observation only to-day in the clinical service of Dr. Lewis at the Orthopædic Hospital.

Case III.—A nervous mother brought her little daughter of seven to learn what was the matter with her. The child was pale and illy nourished, but had no definite complaint. She had fallen about a month ago, striking her nose and suffering a copious epistaxis, which had been repeated some three weeks later. About two weeks ago the child was seized with severe headache, followed by high fever and delirium, continuing for two or three days. After the lapse of a week, the symptoms reappeared, lasting now, however, only throughout the night. Inquiry elicited the fact that two years ago the child had suffered from "congestion of the liver," in the sequence of which she was unable to walk for a period of some weeks. Gradually, with careful training, the power of locomotion returned. There had never been a convulsion or loss of consciousness, and there was no undue laughter or weeping.

Needle-prick on the right hand was less readily appreciated than upon the left; and the point of the needle was said to be felt upon the right side of the face as a piece of hot iron. Some doubt may be felt as to the results of this sensory examination, inasmuch as the responses of the child appeared to be influenced by the tendency of the questions. However this may be, the girl submitted bravely to painful impressions capable of causing an older person to wince.

The knee-jerks were preserved; the gait was normal; the heart was rather overacting, though entirely rythmic, and its sounds were clear. Hypnotism was attempted, and appeared successful. At one time in the course of this procedure the child, when directed by her mother to open her eyes, maintained that she was unable to do so. I have ventured to stamp this case with a diagnosis of hysteria, though fully alive as to its vulnerability and the possibility of deception on the part of both patient and clinician.

Case IV.—S. H., eleven years old, was brought to the clinical service of Dr. Lewis Brinton, at Howard Hospital, by her mother, a music teacher, having two other children, and herself profoundly neurasthenic, with the statement that the child was suffering from spinal trouble, which manifested itself by the appearance of a swelling at the back of the neck. The mother related that she had suffered similarly during her pregnancy with this child, and this condition in her was attributed to "falling of the womb." Both mother and child suffered from time to time from sick headache, with nausea and vomiting. The general condition of the little patient was stated to be sometimes better and sometimes worse. She was said to be quick-tempered and self-willed, and she cried and screamed at times, particularly if not permitted to have her own way; although there had never been a convulsion or loss of consciousness. At times also there was undue laughter. For various reasons, but especially on account of her emotional mobility, the child had not attended school regularly. Her appetite was described as ravenous; the bowels were regular and digestion was good. When headache and nervousness were especially marked the child vomited repeatedly, but improvement ensued in the course of some hours, following the taking of food. Sleep was variable, although as a rule it was good. The knee-jerks were preserved and common, and painful sensibility appeared to be intact. The pupils were full, equal, regular and reactive to light. The action of the heart was rythmic, its sounds clear. Hypnosis was attempted on two occasions, but without success.

I look upon this case as one of hysteria, somewhat ill defined though it be. While it presents none of the obtrusive stigmata of that disease, it is possible that time and further observation may lead to the detection of some one or another of the more characteristic phenomena. The intimate character of a disease is to be learned from a study of its anomalous as well as of its more typical forms. Neurotic parents are capable of exerting a deleterious influence upon their offspring through both heredity and association.

Case V.—L. S., a colored girl, fourteen years old, complained of pain and soreness in the region of stomach, which proved to be sensitive to touch. She was unable to skip and jump and play like other children on account of the resulting distress. She suffered from nausea occasionally, unattended, however, with vomiting. There was complaint of a good deal of headache, although correcting glasses were worn. There were also present tick-like movements of the eyelids. The appetite was good, the tongue coated, the bowels constipated. Menstruation had appeared for the first time some nine months previously, and although a little irregular was unattended with pain. During the preceding month there had been suppression of urine for three or four days on two or three occasions. The urine itself was said to be clear and yellow. For two years the girl was subject to what were described as faints, in which she did not fall, though she seemed to lose consciousness. In these she clenched her fists, and on one or two occasions she kicked, but she had no well-defined convulsion. The duration of the attacks was said to range from five to thirty minutes. At the conclusion of the attack the child seemed exhausted and she felt drowsy. There was at no time undue laughter or causeless weeping. The attacks recurred two or three times a week, at intervals of two or three weeks. There had been a free interval of as long as three months. No definite cause could be assigned for the attacks, though they were associated in the mind of the mother with the function of menstruation, to which, however, their frequency bore no apparent relation. Nothing also was known that seemed to be capable of inhibiting or absorbing the attacks. The child did fairly well at school, although apparently without ambition. There was no history of similar disease or of other form of nervous disorder in the family. The child had never suffered from any serious illness. She was considered sensitive. The knee-jerks were preserved, the patient jumping when the patellar tendon was struck and also in anticipation of a blow that was threatened but not struck. There was no gross sensory derangement. The heart displayed no abnormality. Dr. C. Y. White, by whom the patient was referred to Dr. Brinton's clinic at Howard Hospital, informed me that the patient was susceptible to hypnotism, but her failure to return prevented further study of the case.

Although I believe this case to be one of hysteria, it would appear to be the part of wisdom to withhold for the present a final diagnosis, in order that further study and perhaps personal observation of one of the attacks may yield evidence of such a character as will permit of an unequivocal and unreserved conclusion.

Case VI.—R. F., a schoolgirl, fourteen years old, applied at the Orthopædic Hospital and Infirmary for Nervous Diseases, in the clinical service of Dr. S. Weir Mitchell, on July 3, 1896, with a history having been frightened some eight months before by masqueraders. She fell to the ground and was unconscious for three hours. Her jaws were locked and she breathed heavily. After consciousness returned she went to sleep and remained in bed for several days on account of weakness. Some two months later, following a recitation, the girl lay down and became unconscious and exhibited occasional twitches of the muscles. This attack lasted for three or four hours, and again several days were spent in bed. A third attack occurred after an interval of three months, without known cause. In this also the girl lay down, appeared unconscious, with her eyes open, and became rigid, but she did not bite her tongue. This attack continued for four or five days, during which the patient regained consciousness at times for periods of fifteen minutes.

At other times she appeared as if dead, being unable to see or talk. She took a little food, both liquid and solid. At the time of application the attacks recurred almost weekly, lasting for three or four days at a time. The seizures were characterized by rigidity rather than active movement. After some of the earlier attacks there was inability to see, walk or talk. In some of the attacks the patient scratched herself, in some she pulled her hair, in some she kicked at those about her, and in some she made attempts to bite. In some she groaned a good deal, and in others the rectal and vesical contents were passed incontinently. Some of the attacks had occurred while the patient was alone and some at night in bed. Between the attacks she was often dull, despondent and uneasy. At times, and especially following the attacks, she cried freely and at times she laughed unduly. At times, further, she was unusually obstinate. Appetite, digestion and sleep were good, and the bowels were regular. The knee-jerks were preserved and the pupils were full, equal, regular and reactive to light. There appeared to be general diminution of sensibility to pin-prick. Of the family history it need only be said that the mother, fifty-three years old, was a nervous invalid. The patient herself had had measles and whooping-cough in childhood, and from time to time suffered from bilious attacks. Menstruation appeared first at the age of thirteen and was irregular and painful. In the inter-menstrual periods pain and swelling over the left ovarian region were complained of. Prior to the present illness there had never been a convulsion.

The hysterical nature of this case appears so obvious that further comment seems uncalled for.

Case VII.—M. M., a schoolgirl, fourteen years old, applied at the Orthopædic Hospital and Infirmary for Nervous Diseases, on September 14, 1896, in the clinical service of Dr. Wharton Sinkler, on account of a coarse, rapid tremor of the right upper extremity, most pronounced in the hand, which had made its first appearance without known cause, a month before, ceasing after a week and being resumed after an interval of three weeks. The movement was least marked during rest and appeared to be increased on voluntary movement. It ceased during sleep. No derangement of sensibility and no limitation of the visual fields could be made out on superficial testing. The dynamometric record was 6 on the right and 23 on the left. The knee-jerks were feeble. The mother of the child was distinctly neurotic. The little patient herself had had four attacks of chorea, two involving the right side and two the left, the first at the age of six years and the last at the age of nine.

Under date of June 21, 1897, it is noted that the tremor has disappeared and reappeared thrice since the previous record. Both the girl and her mother consider her well at present, although on investigation it is found that the right hand is tremulous when extended, and inquiry reveals the fact that the shaking appears on excitement or after muscular activity, but only, or at least only in marked degree, in the right hand. There was no impairment of painful sensibility.

This case illustrates one of the forms of motor disturbance that may attend hysteria. In addition to simple tremor there may be choreoid, or tic-like movements, or spasm or convulsion, or paralysis or paresis.

Case VIII.—In conclusion I wish to refer briefly to a rather remarkable case, in a girl of sixteen and a half years, in whom hysterical manifestations had been present for three years or more. The patient's mother was distinctly neurotic and the father suffered probably from some organic disorder of the brain. At the age of thirteen, after some opposition, the girl fell to the ground and became rigid and blue and so remained for perhaps half an hour. Subsequently she wept. In the following year she felt certain vague sensations, with perhaps some perversion of consciousness, after witnessing an accident in the laundry in which she was at the time employed. Some months later she was found wandering about at a distance of some six miles from home, and another month later at a distance of eighteen miles or more. Menstruation set in shortly after this last escapade and recurred irregularly, with pain. At about this time the girl began to have staring attacks, which were attended with convulsive movements. In the following year she did fairly well, but at the end of this time she again walked away from home a distance of eighteen miles. About a month afterward, in conjunction with some sort of seizure from which her father suffered, the girl slept for four days, taking only liquid but no solid food. About four months later she went irresponsibly to Washington, D. C., and passed through a varied experience. Of the details of these several expeditions the girl maintained she had little or no knowledge and only faint and ill-defined recollection. She was readily susceptible to hypnotism, and while somnolent numerous facts and incidents connected with the journey to Washington and her sojourn there for several days were elicited. There was diminished sensibility to pin-prick upon face, hands and legs in irregular distribution. The pharyngeal reflex was preserved. From recent information I learn that the girl was a short time ago found under suspicious circumstances in a not entirely reputable neighborhood, and she been since sent to a reformatory institution. At the time the patient was under my observation I could not convince myself of the entire reliability of her statements, and I am yet unable to decide to what extent she endeavored to practise simulation or deception. I was and am still inclined to believe that many of the symptoms have an hysterical basis, and I would be loath to deny that the several escapades were manifestations of a form of modified consciousness. In addition to the nervous disease in her own immediate family, the fact that an aunt is a mesmerist and has exercised some influence over the patient is not without interest.

I have in this communication endeavored by the report of cases to supplement what others have already done in directing general professional attention to the liability of children to suffer from hysteria, and I would further emphasize the importance of its early recognition and intelligent treatment.

My thanks are due Drs. Mitchell, Sinkler, Lewis and Brinton for their kindness in permitting me to make use of their cases.

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SELECTIONS.

THE BACILLUS OF YELLOW FEVER.

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I.

No one watching a case of yellow fever, and noting with attention the successive development of the striking symptomatology which gives this disease so typical and characteristic a physiognomy, can for a moment doubt that the whole process is due to some absolutely specific microbial infection, one therefore that can easily be verified by post-mortem examination.

Yet, in the majority of cases, even the most complete and minute bacteriological researches, made on the cadaver, only end in entirely disconcerting the investigator and, to a certain extent, in discouraging him. Thus cadavers of the victims of icteroid typhus either remain sterile or are invaded throughout by various species of bacilli, such as the streptococcus, the staphylococcus pyogenes, the coli-bacillus, the proteus, etc., which can in no way be looked upon as the true causes of the disease. In other cases again, they present such a mixture of microbes that their separation, their classification and their study would entail an amount of labor which in itself would render all careful systematic investigation impossible.

It would be useless to give all the technical details by which I have been enabled to recognize the bacillus of yellow fever; be it enough to say that I met with this good fortune in the second case which I had occasion to examine at the Island of Flores Lazaretto. This case, unlike the first, in which I had only come upon a mixture of different microbes, showed the specific bacillus in a state of comparative purity, and I at once gave it the significant, if perhaps slightly improper, name of bacillus icteroides in view of the fact that yellow fever is also described as typhus icterodes or icteroides. I have advisedly employed the phrase " in a state of comparative purity," because yellow fever is the type of diseases with mixed infections: in not one, out of the eleven post-mortem examinations I made, did I meet with the bacillus icteroides alone, it being always associated, either with the coli-bacillus, the staphylococcus or the streptococcus. In the second case observed at the Island of Flores it was associated with a few colli-bacilli alone, and in the eighth case, observed in Rio Janeiro, with the staphylococcus aureus.

In every other case I either met with it in insignificant numbers in the midst of numerous other species of common microbes, or altogether failed in detecting it, the cadaver having already been completely invaded by other microbes, the proliferation of which had been facilitated by the preparatory action of the bacillus icteroides, the latter being thereby prevented from developing or even destroyed entirely.

The bacillus icteroides must be looked for in the blood and in the tissues, not in the gastro-intestinal cavity, where it is never met with, although this would a priori appear to be a likely localization. As a matter of fact, in yellow fever as in typhoid fever, it is the bacillus coli which swarms in the digestive tract, and is there met with in a state of almost absolute purity.

My researches show that the specific germ of yellow fever can only be isolated in 58 per cent. of the cases, and in a few rare instances it may even be obtained during life.

It is easy to understand the reason why it is not possible to isolate the specific microbe in every case of yellow fever. In the first place, when the disease is just beginning, the icteroid bacillus multiplies but sparingly in the human organism and, as will appear further on, a small quantity of toxin is sufficient to determine in man the complete and very severe complexus of the disease.

Secondly, the toxin, acting either directly as such, or indirectly through the deep lesions which it causes in various organs, but more particularly in the digestive mucous membrane and in the liver, notably facilitates the development of secondary infections of every description. These secondary infections occasionally assume the character of true colibacillary, streptococcic, staphylococcic or some other form of septicæmia, capable in itself of killing the patient. At other times again, they form mixed associations of such complexity that the patient may positively during the last stages of his life become transformed into a culture of almost every species of intestinal microbe.

Lastly, my researches having shown that the bacillus icteroides is found, circulating in the blood and various organs, whereas it is never possible to bring it out in the gastro-intestinal contents, it must be assumed, contrarily to the hitherto prevailing opinion, that the virus of yellow fever does not inhabit the digestive tract, and that, therefore, its poison is not absorbed through the intestinal walls, but elaborated in the organs and in the blood itself.

II.

From the morphological point of view, the bacillus presents no special points which may be looked upon as pathognomonic: it is a small rod with rounded ends, occurring in pairs in the cultures, in small groups in the tissues; it measures from 2 to 4 u in length and is two or three times longer than it is broad; it is rather polymorphous.

The search for it in the tissues is successful only in cases in which the patient has died without any secondary septicæmia. Even when the bacteriological result is most conclusive, it is not easy to bring out the icteroid bacilli in sections of the tissues, their number being often very small. It is possible, however, to detect the bacilli in the organs, generally in small groups, in the capillaries of the liver, the kidneys, etc.

The best way of showing, not only the presence of the icteroid bacilli, but also their tendency to localization in small groups, more particularly in the capillaries, consists in taking a small portion of the liver from a fresh cadaver, and, keeping it for twelve hours in a stove at a temperature of 37° C., so as to help the proliferation of the specific organisms.

The yellow fever bacillus develops very fairly on all ordinary nutritive media.

In plate cultures on ordinary gelatin, it forms rounded, transparent, granular colonies, which for the first three or four days present the aspect of leucocytes; later on, the granulation of the colony becomes more marked, and, as a rule, a completely opaque central or peripheral nucleus gradually develops. In time the whole of the colony becomes entirely opaque; it never liquefies the gelatin

Streak cultures on obliquely solidified gelatin develop in the shape of opaque glistening droplets, like drops of milk.

In broth, the icteroid bacillus develops sparingly and never forms a pellicle or a flaky deposit.

On solidified blood-serum, its growth is almost imperceptible.

Culture on agar, unlike what obtains for almost every other known pathogenic microbe, supplies us with an excellent means of diagnosis, under certain special conditions.

When the colonies develop in the incubator, their aspect does not differ from that of many other microbal species; they are rounded, greyish, slightly iridescent, transparent, with a smooth surface and regular margins. If then, instead of growing them in an incubator at 37° C., we leave them to sprout at a temperature of 26° to 22° C., the colonies take on the aspect of drops of milk, opaque, elevated, with a pearly luster, quite different therefore from the stove-colonies.

This difference in the two modes of development may be taken advantage of for diagnostic purposes, and the cultures may be put for twelve or sixteen hours at the temperature of the incubator, and then for twelve or sixteen hours be left at the ordinary temperature of the air.

The colonies are then seen to be constituted by a central, flattened, transparent nucleus of a bluish tinge, surrounded by an opaque projecting rim, the whole presenting the aspect of a sealing-wax impression.

This character may for the present be regarded as specific. It can be brought out in less than twenty-four hours and may, therefore, be made use of for the rapid and safe bacteriological diagnosis of the icteroid bacillus. In addition to this morphological character, which allows of the diagnosis of the bacillus of yellow fever from all other bacilli known, the icteroid bacillus also exhibits various interesting biological peculiarities.

It is an optional anaërobic germ; it does not resist Gram's staining fluid, it sets up slight fermentation in a solution of milk sugar, a more active one in grape and in cane sugar, but is incapable of coagulating milk. It stands a considerable degree of desiccation, but dies in water at a temperature of 60° C., and under the rays of the sun after an exposure of seven hours; it survives a long time in sea-water.

III.

The specific germ of yellow fever is pathogenic to the majority of domestic animals; few microbes have such a wide and varied pathological field.

Birds are completely refractory to its action, but all the mammals I have experimented with proved sensitive, in varying degrees, to its pathogenic action.

In mice, guinea-pigs and rabbits it produces a cyclic affection which is analogous to what is met with in the human subject, lasting nearly five days in the first and third species of animals, from eight to twelve days in the second.

The inoculated germs multiply very sparingly in the various organs during the development of the disease, but from twenty-four to forty-eight hours before death they suddenly break into the general circulation and kill the animal by septicæmia.

The steatogenic action of the icteroid bacillus is first observed in the liver of the rabbit.

The disease may also be experimentally transmitted to the guinea-pig and the rabbit through the respiratory organs, and in this case bacteriological examination shows the existence of a toxic process, similar to that observed in the human subject. Possibly, therefore, contagion by the yellow fever microbe takes place in nature through the respiratory organs also; this would be in accordance with the prevalent opinion on the subject.

In the dog, the icteroid bacillus gives rise to symptoms and anatomical lesions which strikingly resemble those observed in man: vomiting, hæmatemesis, hæmaturia, albuminuria, hæmorrhagic gastro-enteritis, nephritis, icterus, deep fatty degeneration of the liver, uræmic intoxication, and a series of secondary infections.

In the monkey, it may produce the cyclic disease, with complete steatosis of the liver, mixed infections, etc.

In the goat and the sheep, it attacks the kidneys still more severely, giving rise to albuminuria and uræmic intoxication; it also determines acute specific degeneration of the hepatic cells, thus favoring the occurrence of mixed infections. The yellow fever bacillus is thus seen to possess three main pathogenic properties, which contribute toward giving it its special character, and may almost be considered as specific;

(a) The steatogenic properties, which manifest themselves with all the greater intensity in proportion as the animal experimented upon is higher in the zoological scale: they are thus seen to be minimum in the rabbit and maximum in the dog, the monkey and man. Jaundice appears, as a rule, when the disease has already reached an advanced stage. It is due, to a large extent, although not exclusively, to the deep anatomical alterations of the liver, in which dislocation of the hepatic trabeculæ must constitute a mechanical obstacle to the flow of bile and thus favor its absorption by the lymphatics;

(b) The congestive and hæmorrhagiparous properties, which, although common to various species of microbes, constitute, owing to the anatomical parts where they are more particularly localized, a very remarkable specific character. To them are due, not only the classic vomiting of blood (vomito negro) and the various hæmorrhagic accidents, which are common in the mucous membranes, but also the congestion of the vessels, which is the principal cause of the pathognomonic pain of yellow fever (cephalalgia, rachialgia, hepatalgia, etc.);

(c) The emetic properties, which, although not so distinctly specific as the foregoing, impart to this microbe, through the rapidity, the intensity and the persistence of their manifestation in man and the higher animals (the dog), a very peculiar pathological character, by which it is readily distinguished from all other known microbes.

IV.

I have hitherto referred to the yellow fever virus and the icteroid bacillus as the direct cause of all the symptoms and all the anatomical lesions of both human and experimental yellow fever.

The comparative rarity of the icteroid bacillus in the human organism, and the severity of the symptoms manifested in the dog, immediately after the intra-venous injection of a culture containing comparatively few germs, are both indicative of the existence of some very active specific toxin. I shall now investigate the properties of this toxin, which, like that of diphtheria, is obtained by simple filtration of a broth culture twenty or twenty-five days old.

The yellow fever toxin bears with apparent impunity a temperature of 70° C., but is appreciably attenuated by boiling heat.

I have studied the action of this specific toxin on the guinea-pig, the rabbit, the dog, the cat, the goat, the ass, the horse, and on man. Its action is slight on such animals as react but slightly to the living microbe, on the small rodents for example, which are only killed by large doses of the toxin, small quantities of it only determining temporary loss of flesh.

In dogs, however, the toxin when injected into the veins gives rise to the same lesions which I have already described in connection with my experiments with the virus. Immediately after the injection, the animal shows no particular symptoms; but within ten or fifteen minutes, it is seized with a general rigor, which continues without interruption, accompanied by a copious lachrymal secretion, and followed by violent and continuous vomiting, at first of food, then of mucus, so that in a few moments the animal has entirely emptied its stomach and falls down helpless; often there is also early hæmaturia.

If the dose injected is moderate, the dog gets well again rather quickly after this severe attack, which can be compared to the poisoning resulting from an overdose of an emetic; if, however, the dose of toxin be large enough, or if the injection be repeated on the following days in progressively increasing doses, the dog finally dies with the anatomical lesions described as resulting from the action of the living microbe.

These lesions consist in copious pleural hæmoglobinic exudation, marked fatty degeneration of the liver, acute parenchymatous nephritis, albuminuria, hæmaturia and gastric hæmorrhage.

Bacteriological examination also proves of high interest, showing, as it does, the presence of mixed infections, as in ordinary cases, due to the bacillus coli, the streptococcus and the staphylococcus.

The cat is endowed with high resisting power both to the virus and to the icteroid toxin; perfectly enormous doses of the one or the other may be injected into the animal without producing any result, except more or less marked loss of weight, with inflammation and suppuration at the seat of the injection.

In goats, the icteroid toxin determines exactly the same lesions as in the dog and in man, but there is no vomiting. The most noteworthy point in the case of goats is the great tendency to hæmatolysis (hæmoglobinic exudation, hæmoglobinuria), and the extreme sensitiveness of the kidneys to the yellow fever toxin. The death of the animal is due largely to the profound alteration of this viscus, the great quantity of urea met with in the different fluids of the organism constituting strong presumptive evidence in favor of a marked uræmic intoxication.

I have experimented on a single ass: the same morbid complexus occurred in this animal also. There supervened inflammation and degeneration of the liver and kidneys, lesions of the mucous membranes, hæmorrhage in various parenchymata, in serous and mucous membranes, and in glandular organs, such as the mamma, lastly, the terminal phenomena of uræmia and of invasion of the organism by various species of microbes.

Lastly, the horse is extraordinarily sensitive, even to the action of small doses of this toxin. The subcutaneous injection of small doses of a filtered culture invariably gives rise to marked local swelling, with fever lasting from twelve to fourteen hours. The swelling itself is intensely painful, and diminishes very slowly.

When the injection is more abundant and is made, not with a filtered culture, but with one which has been sterilized by the addition of ether, and is much more active than the first, the swelling is of larger size and is invariably attended with extensive subcutaneous œdema, which spreads to the dependent portions of the abdomen and to the limbs, often preventing all movements of the joints for several days. The œdema and the swelling last some days, during which the animal is often feverish.

Intravenous injections are better borne, but are not without serious drawbacks. After each injection the animal has an attack of severe dyspnœa and presents general trembling which compels it to throw itself down; fever sets in and the animal lies completely helpless and listless; the following day, however, the temperature becomes normal again and the horse usually recovers.

In the course of my vaccination experiments I have lost several horses, one of these belonging to the creole breed, which is much less resistant than the half-breeds to toxins in general, and in particular to the diphtheritic and amarillic toxins. This creole horse, a few hours before death, had had a few distant attacks of enterorrhagia; on post-mortem examination the spleen was found to be considerably enlarged; there was in addition a slight degree of degeneration of the liver, nephritis, albuminuria and several small foci of enteritis.

This sums up my observations on the action on animals of the icteroid toxin. I do not think it would be advantageous to dwell longer on these researches, because the result would simply be a reproduction, with varying intensity of the lesions already described, and because I have deemed it preferable to determine peremptorily, and finally, by direct experiments on man, what are the specific functions of the yellow fever toxin.

V.

These experiments are the most important and the most conclusive of all, as they definitely establish the specificity of the micro-organism which I have discovered, and throw light on the hidden mechanism of the mode of action of this bacillus on man.

In respect to yellow fever, as well as of other infectious diseases, courageous men have been found, even at a time when we had as yet no notion of its specific microbe, who did not hesitate to submit to personal experiments, some with the object of disproving, others of proving, the contagiousness of the disease.

Thus Chervin, of Pointe-à-Pitre, in 1816, repeatedly swallowed large quantities of vomito negro without experiencing any ill effects. A few years previously, the North American practitioners Potter, Firth, Cathrall and Parker had tried every means in their power to inoculate themselves with yellow fever. They first tried, unsuccessfully, several experiments on animals, then inoculated themselves with the black matter immediately after it had been vomited by the patient, placing it in their eyes and in wounds made on their arms, breathed in the effluvia evaporating from it, made it into pills which they swallowed, inoculated themselves with the saliva and the sweat of the patients, in a word tried every imaginable means of taking the disease experimentally. All their efforts remained fruitless, and for many years afterwards the general opinion in the United States was that this terrible disease was not contagious.

We are better able to-day to explain the true cause of these unexpected failures. Medical men of the beginning of the century believed, as many do to this very day, that the yellow fever poison must exist in the vomito negro, and thus persistently went on experimenting, as a rule, with this liquid. As a matter of fact, however, as I have explained, the icteroid bacillus is not usually found in the stomach. and if perchance it does reach there, along with the blood, during an attack of hæmorrhage, it can only be in a state of great dilution, as in the circulating blood itself.

My experiments on human subjects are five in number; for obvious reasons I did not employ living cultures, but one made in broth, from fifteen to twenty days old, filtered through Chamberland's filter and still further sterilized, so as to make it quite safe, with a few drops of formic aldehyde.

I tried subcutaneous injections in two individuals, intravenous ones in the other three. These experiments, although not very numerous, proved so successful as to throw an altogether unexpected amount of light on the whole morbid mechanism of icteroid typhus.

The injection into man of even a comparatively small dose of the filtered culture produces a typical attack of yellow fever, with its striking anatomical and symptomatic accompaniments.

I have seen developed under my own eyes, and under the powerful influence of the yellow fever toxin manufactured in my artificial cultures, the fever, the various congestions and hæmorrhages, the vomiting, the fatty degeneration of the liver, the headache, the rachialgia, the nephritis, the anuria, the uræmia, the jaundice, the delirium, the collapse, in a word the whole of the symptomatic and anatomical elements which, when grouped together, constitute the basis of our diagnosis of yellow fever.

This fact not only is a very convincing demonstration of the specific value of the icteroid bacillus, and a success of the first rank in the field, already so rich, of experimental science, it also establishes on new foundations our conception of the ætiology and the pathogenesis of yellow fever.

When we have thus got rid of the prevailing theory, according to which the digestive tract and the stomach in particular are the main center of amarillism, owing to the fact that it is the gastro-intestinal phenomena which especially attract the attention of the clinical observer, and when it has been established that all these imposing phenomena are simply due to the poison secreted by the micro-organism, circulating in the blood, yellow fever at once takes its rank in the class of diseases in which I have for some time past placed another morbid entity, the true significance of which was but imperfectly understood before my investigations, viz., typhoid fever.

All the symptomatic phenomena, all the functional changes, all the anatomical lesions of yellow fever are the result simply of the steatogenic, the emetic and the hæmolytic action of the toxic substance secreted by the icteroid bacillus. Yellow fever again, owing to its general symptoms, its ataxo-adynamic manifestations, its tendency to hæmorrhage, to jaundice, etc., has been compared to poisoning from certain snake-bites. A further point of resemblance between the two morbid processes is found in the existence in both of the hæmorrhagic gastro-enteritis which is still erroneously attributed, in the case of poisoning, to an effort at elimination on the part of the organism.

VI.

When we have thus eliminated the mode of penetration and the seat of election, all arbitrary, which the older doctrines ascribed to the specific micro-organism of icteroid typhus, we must yet admit that it is not easy to ascertain with precision the way in which it really penetrates into the organism. Observers in yellow-fever countries have not, so far, obtained sufficiently conclusive evidence of the hydric theory of the transmission of the germ. Numerous cases, on the other hand, argue in favor of its atmospheric transmission. Indeed, the remarkable resistance of the icteroid bacillus, both to desiccation and to the action of water, as I have ascertained, would make it likely that the yellow-fever virus might be equally well disseminated both by air and by water.

Experiments made on animals, on the other hand, show very clearly that the contagion may take place through the respiratory organs.

As regards the mechanism of contagion by water, it is an acknowledged fact at present that the epithelium of the digestive tract, when intact, does not, as a rule, allow of the passage of any morbific germ. We must keep in mind, however, that, in yellow-fever countries, the slightest disturbance of the digestive functions, brought on by excessive absorption of alcoholic or iced drinks, indigestion, indiscreet use of fruit, etc., more particularly in people who have but recently arrived in the country, like all depressing factors in general, constitute a determining cause capable of ushering in the fever.

We must also remember that new-comers in tropical countries often suffer from slight catarrh of the bile-ducts; this, coupled with the inevitable strain on the liver, may possibly help the icteroid bacillus, by whatever channel it may have got into the intestine, in fixing itself at some part of the hepatic organ.

The formidable effects of the amarillic poison being thus well known, we are in a better position to understand how the germ is able to persist, and even to multiply, in an organism in which it has succeeded in fixing itself.

Nothing is easier for the icteroid bacillus than to gain access into the intestine, if we consider it as forming part of the ordinary microbial flora of countries in which yellow fever is endemic.

The extreme frequency of hepatic lesions in hot countries would thus constitute a very decided predisposing cause to infection by the yellow-fever bacillus, and this in turn, when it had effected an entrance, would become the main cause of the secondary infections which occasionally impress such a complicated physiognomy upon the bacteriological results of yellow fever, and contribute largely in increasing the already high death-rate from this disease.

I have already discussed at sufficient length the secondary infections, which have so long hidden from view the true specific agent of yellow fever. Experimental investigation of these secondary infections has revealed biological phenomena which throw light on the relationship between the morbific agent of yellow fever and those of the secondary infections.

The icteroid bacillus, either by its specific poison or by the serious hepatic lesions which result more immediately from its action, at a certain moment favors the penetration of the septic germs into the organism; these not only kill the patient much quicker than the specific agent alone might have done, but they also do harm to the latter by invading its territories, impairing its proliferating faculties and indeed its very vitality. The phenomena of microbial antagonism between the amarilligenous bacillus and the bacteria of secondary infection, far from proving useful to the patient—their field of action—simply hasten his death.

VII.

It is well known that yellow fever, once it has gained hold of a ship, will stick to her long and tenaciously; it locates itself more particularly in the hold, in the store-rooms, the cargo, in short, in every close and narrow nook. It is generally admitted that old vessels are the very worst for service in regions where yellow fever prevails; writers on naval hygiene look upon insufficiently ventilated ships, with narrow port-holes, vitiated air stagnating in the upper portions, dampness and bad smells in the lower portions, as types of what they call yellow fever boats.

Damp, heat, darkness and want of air appear, therefore, to constitute the most favorable conditions for the preservation of the icteroid bacillus. In the present state of our knowledge, however, we must not be too ready in ascribing any specific value to these various elements, which after all represent conditions which may prove favorable to the development of all micro-organisms in general. We must, therefore, look for some other concomitant element which favors, almost specifically, the maritime habitat of the icteroid bacillus.

In the course of these studies my attention has on several occasions been drawn to a very simple phenomenon, which appears to me to explain the probable cause of the mysterious longevity and resistance of the icteroid bacillus on board ship: this phenomenon consists in the fact that the common atmospheric moulds favor the development of the icteroid bacillus.

This curious property of moulds, with regard to the yellow fever bacillus can be experimentally demonstrated by simply laying the spores of any common mould on a plate of gelatin into which icteroid bacilli had previously been introduced, but remained sterile, as is often the case. As soon as the mould begins to develop its mycelium, small punctiform colonies of the icteroid bacillus are seen to sprout all round it on the gelatin.

Gradually, as the mould grows, these colonies become more numerous and rapidly increase in size, still keeping round the center formed by the mould.

After a few days the plates of gelatin, on which the moulds have developed either accidentally or intentionally, present an extremely curious aspect; the colonies of the icteroid bacillus, which one would think were now dead or at any rate had long been incapable of all growth, are seen to form around each mould a kind of constellation, which is thicker the nearer it is to the spot occupied by the mould. It would seem then that the moulds possess a zone of influence, as it were, and that the icteroid colonies can only develop in their immediate neighborhood. Outside of this zone of influence, which is always clearly defined, the development of the amarillic colonies suddenly ceases.

This rare case of parasitism, which one might define borrowing the means of existence, this curious form of microbial saprophytic life, probably constitutes the main cause of the easy acclimatization of yellow fever on board ship.

Be this as it may, we must look upon moulds as the natural protectors of the specific agent of yellow fever; through their intervention alone is the latter enabled to live and to multiply on soil which is in itself unsuited for it, or in a temperature which is injurious to its vital activities, and would otherwise be quite sufficient to render its very existence impossible.

The intervention of this apparently very insignificant factor is thus seen to constitute the main cause of the acclimatization of yellow fever, not only on board ship, but also in various regions where it appears to meet with conditions extremely favorable to its development.

VIII.

I have thus examined the specific agent of yellow fever, I have minutely studied its life, its habits, its needs, its relations with external agents and with other micro-organisms, and have been able to elucidate the complicated mechanism of its morbid manifestations inside the human organism; it will now be comparatively easy to deduct from these researches various useful indications for public prophylaxis, and even the clinical treatment, of the disease.

The principal foundation of public defense against infectious diseases consists in a knowledge of their specific cause. Asiatic cholera, typhoid fever, and many other contagious affections no longer now inspire the awe of bye-gone ages, because science, taking its ground on the study of their pathogenic agents, is able to prevent their diffusion and their acclimatization by means of various appropriate sanitary measures and various improvements in the surrounding hygienic conditions.

Apart, however, from the prophylactic ideal, which is hygienically of paramount importance, for it is always better and easier to prevent diseases than to cure them, there is also a therapeutic ideal.

Given the nature of the morbid process in question, I do not apprehend any insuperable difficulties in realizing this latter ideal, and I hope that it will soon be possible to apply to human subjects a treatment of yellow fever which will be at once specific, preventive and curative.—*The Medical Week.*

PROFESSIONAL CONFIDENCES; WITH ESPECIAL REFERENCE TO THE LEGAL DUTY OF PHYSICIANS IN CASES OF CRIMINAL ABORTION.*

By JOSEPH H. RAYMOND, A.M., M.D.

The attention of the writer was directed to the matter of professional confidences by two cases which occurred in the courts about the same time in the early part of 1896, and which will be referred to somewhat in detail. As matters of law, these cases were entirely different; one was a case of homicide, and the other of alleged libel and slander; and yet there were certain elements in each which suggested a common line of thought and a common inquiry, and which showed that the medical mind is entirely at sea, both here and abroad, when called upon to determine practically a course of action in cases involving professional confidence.

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*Read before the Medical Society of the County of Kings, March, 16, 1897.

The first case occurred in London, and is entitled Kitson vs. Playfair and wife. For the facts which I shall use I am indebted to The Lancet, Vol. I, for 1896.

This was an action brought by Mrs. Arthur Kitson against Dr. W. S. Playfair, Professor of Obstetric Medicine at King's College Hospital, and his wife, for alleged libel and slander. The statement of claim set out that the defendants had charged plaintiff with unchastity. Defendants denied publication of the slander and pleaded privilege.

The counsel for the plaintiff in his opening said that the plaintiff complained that after she had placed herself under the professional care of the defendant not only did he draw an erroneous inference from her symptoms, but he also broke the solemn seal of professional confidence by betraying to a third person knowledge which he had gained, and made a charge against the plaintiff that she had been guilty of adultery.

The defendants did not now make the suggestion that plaintiff had committed adultery, for no plea of justification had been set up, but the defendants had pleaded that the communications complained of were privileged. The answer to that plea would be that no privilege could attach to statements made by a medical man as to matters which came to his knowledge in his professional capacity. Any information which a medical man thus received ought to be absolutely secret.

It is not my purpose at this time to discuss this case at length, but to refer to some features of it which bear upon the subject which is under discussion to-night; and yet, that these features may be understood it will be necessary to briefly state some of the facts in the case.

Mrs. Kitson, the plaintiff, was the wife of Arthur Kitson, a brother of Mrs. Playfair. She lived with her husband in Australia until the fall of 1892, when she went to England, leaving her husband in Australia. She last saw him about the end of September of that year. On February 23, 1894, more than sixteen months after she last saw her husband, Dr. Playfair, in consultation, examined her and found the neck of the womb dilated to the size of a five-shilling piece (one inch and a half). Within the neck of the womb and above it, he found a soft fungated mass, which he at first took to be an intra-uterine cancerous growth. The patient was put under chloroform, and he then found that the mass was not growing from the interior of the womb, and that it was easily scooped out and removed. He found this mass to be a portion of fresh placental tissue of a spongy consistence, and containing fresh blood in its interstices. He formed the opinion that Mrs. Kitson had had a recent miscarriage. In his opinion, the substance removed was certainly not a blighted ovum. It was a piece of fresh placental tissue.

Dr. Murio Williams, who had called Dr. Playfair in consultation, regarded the mass as fresh placental tissue, and its condition indicated to him a three-months' pregnancy.

Dr. Hugh Playfair, Curator of King's College Museum, said the substance was undoubtedly placenta which had only very recently ceased discharging its functions.

Mr. Bland Sutton, Professor of Pathology at King's College, said it was undoubtedly placental tissue, and that it was not possible that it could have been formed in December, 1892, and removed in March, 1894.

Sir John Williams gave testimony substantially to the same effect as these witnesses for Dr. Playfair.

On the other hand, Dr. Herbert Spencer, Professor of Midwifery at University College and Obstetric Physician to University College Hospital, testified that it was consistent with legitimate conception from her husband that the substance produced to him should have been removed from the plaintiff on February 23, after the lapse of time which she stated had occurred since she saw her husband, in October, 1892. The longest time he had read of a placenta being retained had been eighteen months. The case was quoted from Charpentier. Six months was the longest retention he had personally known. Dr. W. S. Playfair subsequently testified that the substance referred to in Charpentier bore no relation to the substance he had removed from the plaintiff.

Incidentally we may note, in passing, that the want of harmony among medical experts is not confined to this side of the water.

As a result of what Dr. Playfair discovered he felt it to be his duty to terminate the social and family relations existing between Mrs. Kitson and his family, and told his wife and Mrs. Kitson's brother-in-law, Sir James Kitson, of his conclusions. This was the ground on which the suit was brought. The jury returned a verdict for the plaintiff, and assessed the damages at £12,000. The case was not appealed, a sum of money, the amount not stated, having been paid over to the plaintiff unconditionally.

Full of interest as this case is from a medical standpoint, no less so is it from the standpoint of medical ethics, and to the testimony bearing on this subject we desire now to refer.

During the trial, Dr. Herbert Spencer stated that there was a rule in the profession that information obtained during professional attendance should be considered confidential; that there were exceptions. For instance, in a case where if a medical man did not divulge a professional secret he would make himself an accessory to a crime. In a court of law, also, he would be bound under oath to divulge professional secrets. The presiding justice asked him if this was a written rule, to which he answered, "It is a generally understood rule."

Sir John Williams stated that there was no hard-and-fast line in the maintenance of professional confidence. As a general rule men should hold inviolate professional confidence. The exceptions to this rule are when there are higher claims upon the man, such as in a court of justice. With regard to crime a medical man is obliged to inform the Public Prosecutor of any crime which has been committed or it is intended to commit. The higher claims that a man's wife and children have upon him justify him in taking every measure to protect them. The secrets obtained in the course of professional attendance are the secrets of the patient, not of the medical man, and yet he is not bound to consult the patient before he divulges them. He is and must be the sole judge, without appeal, of the circumstances under which he should publish this knowledge, although in some instances the courts will decide the circumstances.

At this point in the trial the presiding judge, Mr. Justice Hawkins, asked Sir John Williams the following question: "Suppose a medical man were called in to attend a woman, and in the course of his professional attendance he discovers that she has attempted to procure an abortion. That being a crime under the law, would it be his duty to go and tell the Public Prosecutor?" Sir John's answer was: "The last legal opinion upon that very question, obtained by the Royal College of Physicians, is 'Yes.'" "Then," said the Justice, "all I can say is that it will make me very chary in the selection of my medical man."

Sir William Broadbent, Fellow of the Royal College of Physicians of London, and Senior Censor on the board of that organization, said that the rule is a very strong unwritten law that no confidence made to a medical man shall be divulged. The exception being that physicians are compelled to divulge confidences in a court of law, and sometimes to prevent crimes. In his judgment there are also certain circumstances under which as a man of honor and acting under a strong sense of duty, it might be necessary to divulge what is called a professional secret. He thought one of these exceptions was that referred to by Sir John Williams in favor of wife and family in a case where the medical man has clear convictions. He regarded it as difficult to formulate an abstract opinion, but it was conceivable that it might be necessary to divulge secrets under various circumstances, but the disclosure should only be made in the last resort when it becomes inevitable. If the object can be effected in a way short of divulging a secret, it ought to be done that way.

In his "summing up," or as we should say here, in his "charge," Justice Hawkins said that the plaintiff had brought this action for slander and libel. The charge made against her was that after she had come to England from Australia she had had adulterous intercourse with some person, the result of which was a miscarriage. To that the defendant had pleaded, first of all, that the words were not spoken or published at all; and, secondly, that if they were, they were published under circumstances which did not afford justification, but which robbed the libel or slander of its malicious character.

He then dealt with the question of privilege. The action was brought for the communication of the libel not only to Mrs. Kitson (Mrs. Playfair?) but to Sir James Kitson, so that the question as between husband and wife was not now before them (the jury). But since the question was discussed in the course of the case he would touch upon it, though what he said must not be taken to be a ruling on the point. The medical profession might, no doubt, discuss among themselves rules for their own guidance, but they had no power to impose the rules they made upon the public. With regard to the question of the exception to the unwritten rule of professional secrecy, he did not altogether agree with the medical evidence on that point. With regard to cases when it was said the doctor should inform the Public Prosecutor, the judge himself might in some cases refuse to commit a medical man for contempt in refusing to reveal confidences. Each case would be governed by the particular circumstances, and the ruling of the judge, deciding no doubt according to the law, would be the test. If the doctor were called in merely to attend a woman and gave her advice, his lordship doubted very much whether he would be justified in going to the police and saying, "I have attended a poor woman who has been trying to procure an abortion." That would be a monstrous cruelty. The rule did not meet his approbation and he hoped it would not meet with the approbation of anybody else. There were cases, no doubt, in which it was obvious that a medical man should inform. He only protested against that rule being said to be applicable to all cases. There was a third objection, namely, a communication between the doctor and his wife and children in order that they might be protected. That required a great deal of consideration, and it was a very delicate question.

So much for the case of Kitson vs. Playfair and wife.

The second case to which I shall refer occurred in our own city. I shall omit the names of the parties concerned, inasmuch as the jury rendered a verdict of not guilty as to the physician defendant, and the indictment was dismissed as to the midwife implicated. Perhaps I can in no better way state the nature of this case than by giving the phraseology of the indictment. It is as follows:

"The Grand Jury of the County of Kings by this indictment accuse A. B. and B. C. of the crime of manslaughter in the first degree, committed as follows:

"The said A. B. and B. C., on or about the first day of February, 1896, in the city of Brooklyn, this County, with force of arms in and upon one C. D., did unlawfully, wilfully, knowingly, and feloniously use and cause to be used, certain instruments, to wit, a catheter and syringe; by inserting the said catheter and syringe into the vagina of the womb of her, the said C. D., and then and there inject a certain portion of hot water from the said syringe into the womb of her, the said C. D., with intent thereby feloniously and unlawfully to procure the miscarriage of her, the said C. D., the same not being then and there necessary to preserve the life of the said C. D. or of the child of which she was then and there pregnant, which caused the death of the said C. D., against the form of the statute in such case made and provided, and against the peace of the People of the State of New York and their dignity."

Signed by the District Attorney.

During the trial, when the defendant was on the witness stand, he was asked whether he did not know that it was his duty to notify the authorities when he was called in to attend a patient on whom he had reason to think an abortion had been practised. The tendency of the question was to lead the jury to believe that it was a general practice for physicians to notify the authorities in such cases, and because the defendant had not given such notification the jury would naturally infer that he had himself been a party to it.

One of the leading surgeons of the city, who had known the defendant for years and who testified in his behalf, was asked the same question, as to what he would have done under the circumstances. He replied that he would have notified the authorities, but said that he could well understand how a physician who had had less experience, or whose mind was occupied with his patient and how to save her life, might overlook the notification. As has been already said, the jury rendered a verdict of not guilty against the physician who was on trial, and the indictment was dismissed as to the midwife.

From a careful study ot these two trials, the one in England and the other in Brooklyn, and from many conversations which the writer has had with judges, lawyers, and physicians, he felt that the whole subject of professional confidences was in a chaotic state. He believed that the members of the medical profession owed it to themselves to ascertain just what were their legal obligations in this regard, more especially in cases of criminal abortion, to the treatment of which every physician is liable to be called at any time. For if it is his duty to notify the authorities and he does not know it and does not do it, then the fact of his neglect may be used to support the suspicion which may be entertained against him of being implicated in the crime.

The Penal Code of the State of New York, in Chapter IV, Sec. 294, defines abortion as follows:

"A person who, with intent thereby to procure the miscarriage of a woman, unless the same is necessary to preserve the life of the woman, or of the child with which she is pregnant, either

"1. Prescribes, supplies or administers to a woman, whether pregnant or not, or advises or causes a woman to take any medicine, drug or substance; or

"2 Uses, or causes to be used, any instrument or other means:

"Is guilty of abortion, and is punishable by imprisonment in a State prison for not more than four years, or in a county jail for not more than one year.

"Sec. 295. A pregnant woman who takes any medicine, drug or substance, or uses or submits to the use of any instrument or other means, with intent thereby to produce her own miscarriage, unless the same is necessary to preserve her life, or that of the child with which she is pregnant, is punishable by imprisonment for not less than one year, or more than four years."

Writing on the subject of abortion, Henry C. Chapman, M. D., in his "Manual of Medical Jurisprudence and Toxicology," says: "The unlawful expulsion of the fœtus constitutes fœticide or criminal abortion. By the term abortion is understood, medically, the expulsion of the fœtus before the seventh calendar month of gestation; that is, before it is viable. After this period the expulsion is called premature labor. Legally, however, such a distinction is not made, the unlawful expulsion of the fœtus at any period of gestation being regarded as an abortion."

Other writers, Edward P. Davis, M. D., among the number, in his "Treatise on Obstetrics," just published, gives the sixth month as the period of viability. This author is, so far as the writer knows, the first to discuss in an obstetric treatise the duty of the physician in the cases which we now have under consideration. At page 535, under "The Jurisprudence of Obstetrics," he says:

"The duty of the reputable physician in a case of criminal abortion is a matter which requires tact and caution. When called to a case of pelvic peritonitis in an unknown woman, no matter what her apparent respectability may be, the physician should keep accurate notes of her condition and his own actions, that he may make a competent witness if called by the commonwealth. A very interesting question has recently arisen as to whether it is the duty of the physician to inform the authorities regarding the commission of a crime, or whether the patient's condition and the criminal abortion form a professional secret which he is not at liberty to divulge. A common-sense view of the case would be that the physician's first duty is to his patient, to save her life if possible, and that his second duty is to cause the detection and punishment of those who have committed the criminal abortion. If the patient has made the effort herself, it is the duty of the physician to save life if possible, and to keep her secret. If, however, she has been operated upon by others, and especially if a fatal issue occurs, he should at once lay the facts before the authorities, and assist in every way in the detection and punishment of the criminal. It is only by prompt and vigorous action by the reputable profession that the criminal abortionists can be driven out."

From the phrase used by Professor Davis, "A very interesting question has recently arisen, etc.," the writer, thinking that there might have been some legal proceeding in Philadelphia which would throw light on the subject, wrote to him and received the following letter:

250 South Twenty-first Street,
Philadelphia, March 5, 1897.

My Dear Dr. Raymond: In reply to your favor of March 1, I am not aware that the profession of Philadelphia has any rules governing it in the matter of professional confidence, except that which regards all knowledge about a patient as sacred and to be kept in the possession of the physician who may acquire it. In the case of criminal abortion, it is not the patient's condition which might possibly be comunicated to the authorities so much as the fact that a crime has been committed. So long as a patient lives, the physician may hesitate to report the commission of this crime, because it will bring his patient into notoriety. If she dies, however, the physician is certainly absolved from his obligation of secrecy. I am not aware that the law of Pennsylvania is explicit upon this subject; it is, I believe, a matter of common law that no one shall wilfully conceal a crime.

The statements in my treatise were suggested from personal observation of these cases. I see constantly patients of this sort, many of whom die, while those who caused the death remain free from discovery or even censure. The Coroner of Philadelphia is active and energetic in the discharge of his duties, and as a rule the medical profession are but too happy to coöperate with him. The difficulties of these cases are sometimes very great, as in one of which I recall, where everything pointed directly to the guilt of a man under whose care a young girl died after abortion; although this man was repeatedly examined by the authorities, he could not be convicted. I shall be greatly obliged to you if you will kindly inform me of the conclusions reached by your Society, and of your own experience and views of the matter. Very truly yours,

Edward P. Davis.

In looking up the subject I have recently come across the following, which I record here as a matter of reference only:

From the "American Year-Book of Medicine and Surgery. Gould. 1897. Page 1183."

"Damages for Malpractice.—Sharp* reports that suit having been brought in West Virginia against two physicians for causing the death of a woman, through an abortion and subsequent celiotomy for the removal of an ovarian cyst, the Court held that the operations in question were personal transactions, and that the defendants could not testify as to anything said by them or done by them, or others, in her (the dead woman's) presence, or as to the condition of the patient as they found it, or anything occurring when she was present. Death having sealed the lips of one party to the transaction, it was held that the law should seal the lips of the other parties. Such a decision leaves the surgeon and physician absolutely unprotected. The jury disagreed."

The Society is to be congratulated on its good fortune in having present to-night as its guests such distinguished jurists as Hon. Willard Bartlett, Justice of the Appellate Division of the Supreme Court, and Hon. Foster L. Backus, District Attorney of Kings County, and it is earnestly to be hoped that the result of this evening's deliberations will be the formulating, under the direction of the Council and the Legislative Committee, rules of action which will be so plain and explicit that the merest tyro in the practice of medicine will know what his duty is in any case he may be called upon to treat. Physicians whose reputation is established might make mistakes which would pass unnoticed, but younger practitioners, whose entrance into professional life is but recent, might, through a lack of knowledge as to what their duty is, have a blight cast upon their professional reputation from which they might never fully recover.

The Society will, we are sure, look upon it as a good omen that in this discussion we are to have the advice and counsel of Judge Bartlett, for when the last medico-legal question, that of medical expert testimony, was before the Society, it was his guiding hand which shaped the action then taken, and which has been the rule of conduct for the profession in this city ever since.

Judge Bartlett: Mr. President and gentlemen:—The question behind all that Dr. Raymond has said, plainly is: What is the moral obligation of the physician who, in his practice, is led to believe that the crime of abortion has been committed, with reference to a disclosure of that knowledge to the public authorities?

Now, I do not venture to rise here as a censor morum; nor shall I venture, in what I have to say, to express any opinion as to the moral obligation of members of the medical profession. It has always seemed to me that it was very important for a person who occupied a judicial position, whose duty it is to settle controversies between other men, not himself to become an active participant in any sort of controversy; and it would ill become a judge to express any opinion, by way of anticipation, as to questions moral or legal which might in the future come before him, or the court of which he is a member, for decision. So far as doubtful questions of law are concerned, I must refrain from expressing any opinion here this evening; but I think perhaps I may give you a few suggestions or a little information as to what is to-day the decided law in the State of New York on some aspects of this question which will guide you in coming to a correct determination as to the moral obligations of your own profession.

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* Jour. Am. Med. Ass'ns, June 6, 1896.

In England the only profession which was protected at the common law—by the law of immemorial custom—so far as what we call privileged communications are concerned, was the legal profession. As far back as the mind of man runneth in the history of law, an attorney, a counselor at law, has always been privileged against any sort of examination in court which would compel him to disclose communications made to him by his client. It was not so in England in the olden time so far as clergymen were concerned; it was not so so far as physicians and surgeons were concerned; it is not so to-day. While the law of England protects the lawyer, even now to-day it affords no protection to the doctor, the surgeon, or the minister, so that when any material question arises—any question arises in which the disclosure of a fact known to a physician or surgeon becomes material in a court of justice—the physician or surgeon has no right under the law to refuse to disclose what he knows, and may be compelled to disclose it under pains and penalties. So that a case like that of Kittson against Playfair is difficult to discuss with advantage unless you realize the fundamental difference between the law of England as it exists and long has existed in this respect, and the law in this country.

Here in America the rule is very different. By a series of statutes enacted now in almost all the States of the Union—statutes dating back in this State many years—the same sort of protection which is extended to lawyers is extended to clergymen and extended to doctors; and in our own State the provisions of the Code of Civil Procedure, which regulates these matters, are almost in the same words with reference to clergymen and physicians, as the provision in respect to lawyers. Our law not only makes it the privilege of the doctor not to speak of those matters which he learns from his patient, and which are necessary to enable him to treat that patient, but provides that he shall not be allowed to disclose them.

Now, what bearing have these statutes on the question which underlies the thesis of Dr. Raymond's paper—the question as to the moral obligation of the physician to disclose circumstances indicating the commission of a crime, which come to his knowledge, acting as a physician? To enable you to answer that, I will refer to the few cases—they are very few—which have been decided by our court of last resort in regard to this matter. It has been decided that where a physician is called upon to attend a woman suffering from the effects of criminal abortion, and that woman dies, the doctor is compellable to testify as to any fact which he may have learned, even from her, concerning the circumstances which indicate that a crime has been committed. There are two cases decided by our Court of Appeals in which this rule has been laid down. The one most familiar probably to you is that of Carlyle Harris. There the question entered incidentally into the case. You know he was indicted for the murder of his wife, Helen Potts—to whom he had been secretly married—by poisoning. A witness was called upon the trial—a physician —an uncle of the victim, who swore that at the instance of the defendant he had treated the dead woman and had removed from her a five-months' child dead. Strenuous objection was made to the admission of this testimony on the ground that it tended to prove that the prisoner was guilty of another crime—a different crime from that for which he was on trial; and also on the ground that it was a violation of the provision of the statute which forbade the disclosure of a professional communication. The trial court overruled both objections, holding that, although it did tend to prove the commission of another crime, it also had a bearing upon the motive of the prisoner to get rid of his wife subsequently; and, holding that the provision of law prohibiting the disclosure of professional communications had no application to such a case—had no application in favor of the man who was charged with murder. But it was said, on the other hand: "The rule of law also is that no such proof can be given unless the patient waives the privilege. The woman is dead; she cannot waive the privilege." The Court made answer in substance: "While it is true she is dead and therefore cannot be heard to waive the privilege, the presumption is that, inasmuch as she is the victim of this man, if she had any option in the matter she would waive the privilege." Therefore, the rule may be regarded as fairly well settled in this State that a physician is compellable by law to disclose what he knows, indicating the commission of criminal abortion, where the victim of that abortion is dead.

How is it, though, in the cases where the victim of the abortion, or the subject, survives? The cases upon that question are even fewer. There are only two, so far as I have been able to ascertain—perhaps the learned District Attorney can cite more than I have been able to find—

but the only one in the court of last resort holds that where the woman upon whom the abortion has been committed, survives, and does not waive the privilege—does not consent to the disclosure—the physician cannot be allowed to testify a single word about it. There is a case—not in the court of last resort—which is difficult to reconcile with the decision to which I refer; but speaking of the decisions of the highest authority, the rule to-day seems to be that where the woman dies, the physician is free to speak; will always be allowed to speak; indeed, is compellable to speak. Where the woman survives, so that the statement from the physician may tend to disgrace her, he cannot be allowed to make that statement without her consent.

Now, gentlemen, if you carefully consider the effect of the rules which are involved in these decisions—the doctrine and principles upon which they must be based—I think you will find very substantial aid in getting at some just and correct rule to guide your own action in these matters. Further than this I prefer not to say.

The President: I beg to introduce District Attorney Backus.

Mr. Backus: Mr. President and gentlemen:—Some of the remarks that I had intended to make will not be necessary, after the Court has charged you. It is the custom to sum up first, and the opposing counsel are liable to advance such contradictory theories that it is necessary to have a distinguished judge to harmonize them and to aid the jury in determining which is the proper theory. It is often neither what the plaintiff's counsel or defendant's counsel has advanced. The law very often as given by the presiding judge is very different law from that given by counsel on either side.

The object, I suppose, of meeting together—the legal profession and the medical profession meeting together here—is that when we meet together in the affairs of life or in the court-room we may have a better understanding with each other.

The rules established for the guidance of physicians, and the rules established for the guidance of ministers and lawyers perhaps it would be as well for me to read:

"A clergyman is prevented from disclosing confessions made to him in his professional character, in the course of discipline, enjoined by the rules of practice of the religious body to which he belongs."

He is prevented from disclosing anything in the way of confessions which are made to him in his professional character, in the course of disclipine.

Perhaps I had better mention the sections for your reference. The section about clergymen is section 833 of the Code of Civil Procedure. By section 835 it is provided that:

"An attorney or counselor at law shall not be allowed to disclose a communication made by his client to him, or his advice given thereon"——

It is often safe enough for a doctor to say what advice he gives his patient, but for the better protection of lawyers it is provided that the lawyer shall not be allowed to tell what he advises. He might get conceited and think his advice good; and he shall not be allowed to tell what advice he gives his client——

"——or his advice given thereon, in the course of his professional employment."

You see, he shall not be allowed to disclose anything.

But as to a physician, by section 834 of the Code of Civil Procedure it is provided, that:

"A person, duly authorized to practise physic or surgery, shall not be allowed to disclose any information which he acquired in attending a patient in a professional capacity, and which was necessary to enable him to act in that capacity."

So that it would seem that a physician is not allowed to disclose what he gets as a necessary acquisition for the purpose of acting in the capacity of a physician. I can see very well how a physician might, in attendance on a patient, get information from an examination of the patient, that an instrument had been used upon the patient—get no information at all from the patient as to how this state of things was brought about—and he might, with a desire to see that justice were done, avoid getting from his patient the information that would be important in assisting the authorities in a prosecution. He might get that information in another way, and as not necessary for him in the performance of his duty. He could not testify, perhaps, but it would be allowable for him to make a disclosure for the purpose of seeing that justice were done.

Now, in the case of the People against Pierson, who was charged with murder, the Court said that:

"The reason of this enactment was in order that the fullest confidence might exist between the patient and his medical adviser."

The revisers, in their note to this section, in which this provision is contained, say:

"The ground on which communications to counsel are privileged is the supposed necessity of a full knowledge of the facts to advise correctly, and to prepare for the proper defense or prosecution of a suit, but surely the necessity of consulting a medical adviser, when life itself may be in jeopardy, is still stronger; and unless such consultations are privileged, men will be incidentally punished by being obliged to suffer the consequences of injuries without relief from medical aid, and without conviction of any offense. It is believed that the proposition in the section is so guarded that it cannot be abused by applying it to cases not intended to be privileged."

And the Court further said:

"Here it is a privilege designed exclusively for the benefit and protection of the patient, which, it is insisted, not without some colorable foundation in the language of the section in question, may be invoked to shield the alleged murderer of the patient from a disclosure of facts which might have a tendency to establish the commission of a murder."

The Court in this case, after citing a number of cases, in saying that the law should be construed looking for its spirit and to meet the mischief and to advance the remedy, not to violate fundamental principles, and that the privileged relations can only exist for lawful and honest purposes, said:

"The spirit which most clearly intended to protect the patient and not to shield one who is charged with his murder; that in such a case the statute is not to be construed as to be used as a weapon of defense by the parties so charged, instead of the protection of its victim."

And that the statute was intended for a humane and benign purpose, not to be used for the protection and shield from the consequences of the deliberate perpetration of crime, but a distinction was made in the case of the People against Murphy, from which I quote, where the patient was living and the disclosure, which tended to convict the person, tended to convict her of a crime, or to bring discredit or disgrace upon her, between her and the physician there was a prohibition.

The subject was so fully discussed by the Judge that I shall refrain from entering upon that discussion. If you examine the law of accessories you will see if a physician is called to a case and he finds that a crime has been committted, there is quite a broad field between the closing of the physician's mouth so that he cannot be allowed to make disclosures, and making the physician responsible as an accessory after the fact. An accessory after the fact is one, who, where a crime has been committed, shields or harbors or aids the person who has committed the crime, or whom he has reason to believe has committed the crime and should be arrested—shields him, or aids him, or harbors him, so that he be not prosecuted. Of course the physician desires to do more than simply avoid prosecution as an accessory after the crime. Now, the question would arise, what would a physician do under certain circumstances; some cases have arisen, and so I would like to refer to one or two hypothetical cases.

Suppose a physician is called to a house where there is a family and a daughter, and suppose the physician on examination finds evidences of a criminal abortion having been performed by somebody. Of course, the physician's first great desire is to find exactly what is the true situation, in order that he may give relief to his patient; and that being the object of his inquiry, suppose that he learns that the patient herself has used any unlawful means for the purpose of producing that condition. Now she must either disclose to him the whole matter, even to the extent not only of disgracing herself but to the extent of making herself liable as a criminal, or the physician must go without the necessary information or the full information upon which he may act. It seems to me the duty of the physician there would be plainly this: that whether she live or die he legally would be prohibited from making a disclosure of her statement to him, and morally he should refrain from it, for the reason that if she is dead no good can be accomplished; she has suffered enough; punishment enough will be provided for her; but if she live, the matter is a matter between her and himself alone, if the whole evidence against her is his own statement alone, it would be plain that his obligation would be to have the matter remain a secret.

But suppose the physician learns that a person wicked of heart, and deserving punishment now and as soon as possible, has been the one who had produced this condition? Is it not true that a physician, or lawyer, or citizen is then bound by a higher obligation, and that this regulation which closes the physician's mouth as a witness so that he cannot testify is not intended to cover the whole scope of this situation? You have known, and you can very well imagine, men so wicked, or people so wicked, that a girl, a modest woman, who has found herself in such a situation as that may have been ruined eternally by them—if not ruined eternally, certainly ruined all the rest of her life—being led under great excitement and under stress of peculiar circumstances, led to the position by designing persons who want more to shield themselves than anything else.

Now, in olden times boys were regarded as belonging to the State. Certain boys were developed in athletics so that they might become soldiers for the protection of the nation; others who belonged to the higher grades or the King's family, were developed in intellect and courage so that they might become statesmen. But here in our own beloved land it seems to me that every boy and girl of ours, every young man and every young woman, belongs to the State for the purpose of protection; for the purpose of production; for the purpose of government —for every citizen here is a king. It seems to me that the obligation of American citizens for a perfect protection of our people, our young men and our young women especially, is the greatest possible. Therefore, if it comes to the knowledge of the physician, or the lawyer, or the minister, or to any citizen that one of our girls has been thus ruined and a crime has been committed upon her by another, it is important, especially in extreme cases, that information should be given by the physician, as well as all others, in some way so that a wicked-hearted person may be brought to justice.

It seems to me that the moral obligation of the physician, therefore, in cases where the act is performed not by his patient but by another, even where his patient is alive, requires that there should be some effort to get her consent and the consent of her family, so that the person who is responsible for such a terrible injury to the patient and for such an injury to the public, shall be brought to justice—for an injury to the woman, an injury to the girl so that she can have no longer a hope of being a mother, but is a ruined woman—such an injury as that requires careful attention by the physician, and some systematic method should be adopted by our physicians and our public prosecutors, so that the best and the uniform system may be carried into effect.

I am glad that Dr. Skene is here to discuss the subject. Would it not be the proper thing to do to have harmony between the public prosecutor and the medical profession, so that there may be this agreement: that if an unlawful act be performed by another upon your patient, some means may be devised, some plan may be devised by which you as the physician may aid in inflicting the proper punishment upon that person. And with that suggestion I think I will close.

Dr. Alex. J. C. Skene: Mr. President and Gentlemen—I understood that to-night one of our members was to be tried before Judge Bartlett, and that the District Attorney would conduct the prosecution, and I was to defend Dr. Raymond. It turns out, however, that all three of them are quite agreed, and that places me in this position—not an enviable one, I assure you—either to say nothing at all, or else join issue with these three very learned and distinguished gentlemen.

The only way that I can continue the discussion is to try and broaden the subject a little. It has been confined, as I understand it, to the question of abortion. Now, I think the subject applies to many more conditions than that with equal force and with equal importance. I have, on my own responsibility, been guided by the law as defined just now by Judge Bartlett, that is, the law in America, although I got my first hints from the law of England. I hold that the physician or surgeon has no right to disclose any fact or information that he obtains from his patients, under any conditions whatsoever. I believe that that is the only ground to take, and it is the only way that a physician can sustain himself in relation to his patients and to the law of the land, and to the moral law. The mistakes that are made and much of the suffering, misery, and sin that follow has arisen in this world, I fancy, from the doctor telling tales out of school, not from concealing and keeping inviolate all his professional confidences. That is the ground I take.

The question is, how can we possibly harmonize our behavior with the law—the moral law—when we meet those exceptions that have been brought before us by both Judge Bartlett and Mr. Backus. I contend, and I believe that it is practicable, that we can fulfil the law if we do precisely as the lawyers do. When a lawyer is called to a client he is bound to do everything in his power to save him, perhaps from just punishment. He may be perfectly confident that his client has been guilty of a great crime, and yet he is not bound by the law to end the trial by making that confession to the court, but he is bound by the rules of his profession to defend his client and to keep his client's secrets in the most careful manner possible—conceal all this from the public and the court. Now, why not extend the same privilege to doctors? I think that it is their right and I do not see how we should be even compelled to give testimony in court that would tend in any way to involve our patients in any crime or even common offense. Not to the same extent, but in a very great degree, we may, by betraying professional confidence, that is, the confidence of patients, bring much disgrace and trouble—we may ruin the reputation of one who is not guilty of any special crime or offense against society; and so we have to be careful in minor matters, not confining ourselves entirely to matters that are criminal.

Let us see how we can fulfil the law and yet maintain the relations that ought to guide us in professional secrecy. Take this question of abortion—and I am obliged to take a little different view of it from the Judge and the District Attorney. One is called to a case of abortion, for example, and, as the District Attorney has pointed out, he may be able to obtain damaging information, not from anything that the patient tells him, but from other evidence that he obtains. Should he expose his patient? The District Attorney knows as well as I do that evidence obtained from a patient, whether from her tongue or the condition of the tissues of the body, should be just as much a professional secret; so the doctor cannot let his tongue loose in that way. But we can all fulfil the law in this way: If a case proves fatal, and that from an abortion, and we do not know the cause from our own knowledge—we may have kept the patient from telling us or the patient may not have volunteered the cause—then we are not bound by any means to give a certificate of mortality, but on the contrary, the matter should at once be turned over to the Health Department and from there go to the District Attorney, and then the matter would be investigated and the guilty one ascertained in all probability, and without any aid on the part of the physician, excepting the doing of his duty. The mistakes and the blunders that have been made by physicians have been in attending cases of produced abortion that have proved fatal, and the doctor has given a certificate of mortality which has got him into trouble in some cases by doing so when he is perfectly innocent. If we bear that point in mind we can escape violation of the law and obey the rules of the profession.

With the law as laid down by the Judge and the District Attorney regarding exceptions, we doctors find ourselves in the position that opposing lawyers often get us into—we feel as if we would be hanged if we do, and certainly be hanged if we don't tell all we know.

Regarding exceptional cases referred to, I may say that there are, or should be, none such in the practice of medicine; we should avoid making them. We are asked to suppose that a dead patient would, if living, waive her privileges and allow the doctor to testify. If we are careful in the management of such cases, such difficulties will never arise. The patient dies and we refuse to give a certificate until the whole thing is investigated—then it can be made all right without any special effort on the part of the doctor. The doctor should never, in my opinion, play the part of a detective, and while he ought to be willing to do all in his power and never miss an opportunity to raise the moral standard of the people, and his own standard, I think there are other ways of doing so than by telling—"peaching" on patients. For example, the case brought up by the District Attorney in this regard, is it the duty of the doctor, when he is called to see a patient who has produced a miscarriage herself without the aid of any outsider, or any professional abortionist—is it not his duty to the people and to the State and to the community to give information in such a case as that? Most assuredly not. If he is the right kind of a man he can give that woman such a scorching that if she lives she will never do it again. He can, if he is a man of any force and moral stamina, punish her even more severely and in a way she will remember longer, than by dragging her into court. Did you ever notice how many of these cases escape by some technicality in the law? A great many of them.

I may refer to other conditions where we are often troubled to know just how to act. Suppose to-night some of you have a burglar come in with a gunshot wound and you have to attend to it, and he may be sufficiently confidential to tell you that he was trying to find his way into a safe, and instead of succeeding encountered a bullet. It is not, in my opinion, the duty of the physician to go to the District Attorney and tell him about that. There are other gentlemen who should be able to find that burglar and deal with him according to the law; it is not the part of the physician to play the policeman in such affairs because he has had the opportunity. So I might go on and name a number of other conditions in which the physician might have a first-rate chance to rid the community, for a time at least, of some scoundrels of either gender, and so do some good; but at the same time he abuses the confidence of his patients, and even if he does it with the consent of the court and lawyers, he is wrong.

The law is curious in its relations to medical men, and let me show you in what way my line of thought is running in this respect. A doctor is treated as a criminal if he fails to report a case of contagious disease, and when he makes his report to the Health Officer he violates one of the rules of his profession if patients so afflicted desire to conceal their condition, and most of them do. It is a breach of confidence for him in his professional capacity to state their condition to the Health Officer, and yet the law compels him to do it. He is bound by law to report scarlet fever, measles, typhoid fever, diphtheria, and small-pox, but he would be driven out of the community if he reported to the Health Officer another equally contagious and infinitely more disgraceful disease—large pox; and so you see we are placed in very difficult positions in that regard.

I have long desired to have the law changed and amended so that all contagious diseases should be included in the list of those we had to report to the health authorities, so that the guilty should suffer the same exposure that the innocent now do. I hope that Judge Bartlett and District Attorney Backus will take this in hand and aid us in obtaining the required relief.

To-night we have had the law laid down to us more definitely than I have ever heard it stated before, and it is really much better for us than I thought it was. I have been pleased to find that the law is so much in our favor as it is. Knowing more of the law, and being better pleased with it than ever before, I shall live in the full determination to pursue the course that I have always done, and that is, of keeping everything, no matter how trivial, as much of a secret as the law will permit. To do that is exceedingly difficult, and yet it should be done. I do not believe that by pursuing that course generally we will lower the standard of morals in this great and glorious City of Churches; on the contrary, I think that we will simply have the District Attorney and all the officers of the government whose business it is to look after crime, watch us a little more closely and relieve us from all responsibility in that way. We can, by making public the fact (as we have a right to do) that the causes of death are obscure in criminal cases, throw into the hands of the authorities questions of law, questions of crime, questions of morality without by any means violating any professional confidence. I am quite satisfied on that point.

With reference to this gentleman in London, this distinguished member of the profession, Sir W. S. Playfair, I think that he made the biggest mistake of his life, for he has lost cast in London to an enormous extent. I know him very well, and I know that his position was one of the best in London; and yet he did the very thing that no medical man should ever do, and that is, he went home and told his wife. (Laughter.) Now, if there is anything—any professional secrets—that you do not want to spread broadcast—for heaven's sake don't tell your wives, or those who are going to become your wives. I notice that there has been a suggestion among the lawyers to amend the old-time phrase at the beginning of legal documents, "Know all Men by these Presents;" and some cunning lawyer said he could abbreviate it, and improve on it, and make it read, "Know one Woman." "How is that?" they said; to which he replied, "Oh, if one woman knows it, all men will know it very soon."

I believe that more suffering, more injury to the general moral tone of the community comes from telling about things that are trivial. There are very few people in this world that like to have their ailments and diseases made public, no matter how innocent they may be, and though they may not be in the least degree responsible for them. So the doctor has to be exceedingly careful to keep to himself everything, the exposure of which might bring even the slightest discomfort to his patient or give the patient's neighbors a chance to discuss them in an unkind way. There is an immense field for thought in this direction.

I have no doubt that Madam Leo Hunter, who employs the most fashionable doctor, may be perfectly willing that all her neighbors may know that he is attending her for an attack of intercostal neuralgia on the left side, which required daily auscultation to see that it is not organic, but purely functional, but these are exceptions. The great majority of right-minded people dislike to have their ailments talked about, and so I think it is incumbent upon the whole of us not only to keep those important secrets with reference to immoralities, but with reference to everything else. It has become the fashion to ventilate diseases, and symptoms, and treatment in the public press, and the doctors sometimes, unfortunately, aid in this; so that we have reports in the paper of just what the pulse of a celebrated or notorious individual is, how he was nourished by rectal enemata, and all that sort of thing, which I do not think tends to elevate the moral tone of the community. The most difficult of all things is to know how to keep a secret. It is easy enough to let them get out and do an endless amount of harm, and sometimes some good. It may help the District Attorney in the prosecution of a case to have the doctor tell him the facts, and all that sort of thing, but it is far more important that the doctor should remember always to keep the law of medical practice inviolate.

Again, there is another point—I am wandering from the subject a little, and yet it is all vital and all comes to us—and that is, how much shall we tell our patients themselves of their ailments; what is our duty from the moral standpoint to the patient that is suffering from some fatal disease; is it well that we should tell them, or is it better that we should keep them in ignorance for a time at least, until it becomes apparent to the patient's self that we are probably keeping them a little in the dark, and then tell them? With families and friends how far can we trifle with or utilize, if you please to put it in that way, our professional information? For example, is it wise when a gentleman who has been off on an excursion—duck shooting, perhaps—comes home and goes off in a wild delirium, is it according to law and the higher ethics that we should tell his wife that he has got plain, ordinary delirium tremens, or give it some technical name that would relieve her feelings from the worry of such a condition? There are many other diseases I might mention that come into families, and is it well that we should let the whole family know just how it is? Is it not our duty to our patient to keep that patient's secret; would it serve any better purpose to expose him? I do not know; but I do know that if we do we violate a very important law.

The question is, should a doctor tell lies to save his patients from suffering? A clergyman asked me that question and I answered him according to my best light on the subject. I said then, as I want to say now, that the doctor should be truthful; he should tell the truth, and nothing but the truth, but many times he should not tell the whole truth.

Again, on the other side, we are apt to be betrayed by our patients very often, and innocently, too. One comes to you and asks, "What is the matter with that patient that you are attending?" You know you are attending such a one by such a name, and if you are unwise enough to acknowledge that you do, then they say, "What is the matter?" and you hesitate to answer, and they say, "I know what is the matter," and the gossip tells her neighbors, and adds that she has her information from the doctor himself. You remember the little paragraph in George Eliot—I find many such characters—two ladies discussing a third one, and one said, "What do you know about Mrs. Jackson?" "What do I know about her? Well, I know her well enough to have seen her legs, and I know she has dropsy." Now, the inquirer will tell you what is the matter with the patient, and if you do not contradict it they will go off and say, "The doctor told me so; I know it is so." It is exceedingly difficult to keep ourselves from betraying our patients, and the trust they have in us. I know one doctor who gave himself a little harmless trouble by being over careful in this way. He made a call in the morning, and as he came out on the sidewalk to enter his carriage, a very well dressed lady stepped up to him and said, "Excuse me, doctor, but is there anything serious the matter in there?" And the doctor said, "Oh, no, madame; nothing at all; one of the members of the family is suffering from a little indigestion, that is all. Good morning;" and he drove off. The next morning he called in the same house, and the lady who encountered him on the sidewalk the day before came in from another room, dangling a piece of white drapery in her arms, and said, "Doctor, is this the kind of dyspepsia that your patients have?"

Dr. Jerome Walker: I would like to ask Mr. Backus a question. In this hypothetical case that he mentions, when he says that the physician is morally, and perhaps legally, obliged to inform about anybody who has produced an abortion upon the woman—suppose he did so, and suppose he had the consent of the patient to do so, though the woman is in part guilty herself, as sometimes women are; what is going to be the effect upon the woman as far as punishment is concerned? If the man is to be punished because he has committed a crime, and the woman has aided and abetted him as an accessory, cannot she be punished? If so, what is gained; how far has the doctor helped her under those circumstances?

Mr. Backus: What is the leading case in this instance? Now, under the law of principles in the case that went to the Court of Appeals, where the woman did consent, under the advice of a woman, to consult with a lawyer, and the lawyer advised her that it would be perfectly safe for her to assist the District Attorney in prosecuting a bad man; she was used as a witness; she was never punished. The man was punished, and it was a very healthy punishment, too. It seems to me that where the woman sees that—I suppose that it would not be very far wrong to say that a woman under such circumstances as that, and especially if she saw that she were going to die, if her dying declaration became very important, it might be a very good thing to get her consent and let the proper authorities take her dying declaration, so that the guilty person might be punished—there have been cases like that; that if this woman is about to die, and the doctor knows it, and that dying declaration would be admitted as testimony in the case, it seems to me it would be a very proper thing to have arrangements made and have the authorities informed, with the consent, possibly, of the lady. Because, if she consent to make a dying declaration to the doctor—if she knows she is going to die—she might want to talk. But in case she is going to live she might see, as they sometimes do in extreme cases of disease or excitement—she might see that it was her duty to aid in the prosecution of crime. Now, why not use her as a witness? It is perfect protection to her if she gives information. If she becomes a witness she could not be prosecuted because her testimony is the foundation of the indictment, and there are no cases in this State, where a patient used as a witness and gave information, where the patient was subsequently indicted upon such information.

I would like to leave this conundrum, and I do not care to have it now discussed. Suppose a doctor is called to a case and finds that somebody else has performed a criminal abortion; and suppose his patient dies, should that doctor ever perform an autopsy, or a partial autopsy, for the purpose of discovering for his information what is the true internal situation? Should he ever perform an autopsy, or a partial autopsy for his enlightenment, or should he allow the authorities to be first at the scene, so that the authorities' physicians can have an autopsy performed, in order that the true state can be learned by those who perform it, and so get the information? I put that inquiry, though it may never happen again; but there was one such case in the city.

Judge Bartlett: I would like to say a word, Mr. President. I am afraid from what Dr. Skene has said, that he possibly misapprehended my statement of one of the decisions, in saying that under the law a doctor was compellable to testify to facts which he learned from his patient, where the result of the abortion had been death. I meant by that, that if the doctor was summoned into court, that if the District Attorney in the course of the prosecution asked the doctor if he knew anything about it, and he was on the witness stand, then he would have to testify. I did not mean that the law imposed upon him the necessity of volunteering to testify as a witness, and possibly Dr. Skene misapprehended me in that respect.

It is not generally understood that our law in regard to privileged communication relates entirely to the action of professional men in court. It may be, and I think it is, and many eminent judges have said it is, a most dishonorable thing for a doctor to go and tell his wife, or his friends and neighbors, what he finds out from his patients which they would wish kept secret, but there is nothing in our law which prohibits that. There is quite a common misapprehension on this subject. All of our law on the subject relates to disclosures on the witness-stand in court.

I would say a word as to the different views that may be entertained of the morality of this question. Take a doctor who is summoned to attend a person who is suffering from poison. The patient dying gives the doctor information necessary in order to enable him to treat the patient, which indicates perfectly plainly that some enemy of the household has been administering poison to this patient. The patient dies. The doctor says nothing about it ever. Thus, perhaps, the poisoner is left to pursue the same course in some other family. Now, of course, the moral aspect of silence in that case is very different from the moral aspect of silence in a case of abortion, where the person who is responsible for it is some young man who himself came to the doctor to save the victim, and who is more solicitous perhaps than the doctor himself for the recovery of the patient; a repentant man, who himself is perhaps made to suffer lifelong grief from the patient's death. In that case the moral aspect of silence is quite different from what it is in the case I first supposed.

Dr. A. J. C. Skene: I have only to say that I thoroughly understood Judge Bartlett's position and meant to convey the idea that I did.

The question raised by the Judge just now I think can be covered by the suggestions I made before, and that is, that in case of poisoning that proves fatal, the doctor cannot and should not give a certificate of mortality, but is obliged to make that a Coroner's case; and then he can testify just as far as he thinks right and proper before the Coroner, giving his reasons for his own diagnosis and the kind of poison employed, and he may make his statement in a way that will not compromise himself, and yet give the Coroner all the information that is necessary. And what I wished to make especially plain was that all those cases of death from any criminal cause, if the doctor will only shift the responsibility from his own shoulders to the Coroner, and thence to the District Attorney, he will succeed in serving the ends of justice, and not violate any of his confidential relations to his patient.

Dr. Chas. Jewett: I regret, Mr. President, that I could not be present at the reading of the paper, and the more so because of the unusual interest of the subject before it. The seemingly conflicting obligations of the physician to his patient and the public frequently give rise to troublesome questions; and Dr. Raymond will have done the profession a service if his efforts shall be instrumental in defining more clearly the legal and ethical relations of the medical practitioner. While I cannot speak to the paper I gladly avail myself of your kind suggestion to add this brief note to the discussion of the subject. I will refer to but one branch of it: that is the one which especially concerns the obstetrician, the duty of the physician to the State when called to treat the victim of a criminal abortion. I speak purely from the ethical, not from the legal standpoint.

In the vast majority of cases that come under the physician's notice the product of conception only is sacrificed, and that in the early months of gestation. Such cases are seldom brought to the attention of the authorities. The attending physician recognizes usually no obligation as well as no right to report them. Here the gravity of the crime seems too lightly estimated, both by the public and by the law-makers. The old-time distinction between the value of the fœtal life before and after quickening still holds in law, if I am rightly informed. This I venture to assume is an error which needs correction. The life of the new human being dates from the moment of conception, and the distinction referred to is without foundation in fact.

When the life of the mother is also sacrificed the crime assumes more serious proportions. In either event, the man of medicine, no less than the man of law, abhors the deed and would gladly bring the criminal to justice. No malefactor is more despised by him than he whose business it is to procure abortions unlawfully for gain. Few opportunities are presented to him to do a greater public service than in helping to secure the conviction of a professional abortionist. Is it his duty in such a case to voluntarily lay the facts before the legal authorities? From the ethical standpoint I think not, certainly not without the consent of his patient. The woman herself is a party to the crime. In the fear of death she has in confidence committed her secret to him with the hope of saving her life. Not only the confidential communication but as well such facts in the case as are obvious to all would not have come to his knowledge but for the confidential relation. To voluntarily disclose them would be a gross breach of professional honor. To not only make public the facts confided to him by his patient, but to assume the rôle of detective and hunt up further evidence, as I have known a physician to do, is, to my mind, the height of professional dishonor.

But these remarks, as already stated, are made merely from the ethical side of the question. Our notions of ethics must go for nothing if they are not in line with the law. It is the first duty of the physician, as of all good citizens, to obey the law.

Dr. J. H. Raymond: Mr. Backus has laid down very clearly, it seems to me, the course for us to pursue in two cases. I wish he would be equally explicit with the third case. He tells us that when we are called upon to treat a woman who has procured an abortion upon herself it is our duty to keep that secret inviolate. He tells us further that when we are called upon to treat a woman upon whom an abortion has been committed by another, our duty is to endeavor to get her consent to give the facts to the authorities, in order that the abortionist may be prosecuted. I would like to ask him what our practice should be if we fail to get that consent.

There is another question I should like to ask. Is it our duty to notify the Coroner in all cases that have not been seen by us within twenty-four hours? That is a common practice. Has the Coroner any right to hold an inquest in a case which has been seen by a physician, and in regard to the cause of death of which the physician is perfectly satisfied and is perfectly competent to give a death certificate, even though the period of time may have been weeks or months? I would like very much to hear Judge Bartlett's opinion on that point. It would be of great value.

Judge Bartlett: I have lectured about that at the college, I think. Some other time; not to-night.

Dr. Raymond: I know you have, and I wish you would lecture to us here, because it is a matter of very great interest.

Besides the cases which have been mentioned here to-night, there are others which come to physicians about which there is a good deal of doubt. A physician told me the other day that a patient came into his office complaining of some trouble about the mouth. When he examined him he found mucous patches; the patient was suffering from syphilis. He said to him, "What is your business?" He answered, "I am a butter taster." The doctor said, "Tell me just how is that practice carried on?" "Well," he said, "you know we have a long instrument with a handle which we plunge into the butter tub, and pull it out, and draw it through the mouth and taste the butter." "Are there other men in the same employ with you?" "Oh, yes; there are four or five of us." "Do you all use the same butter taster?" "Yes, but every man cleans it after he uses it." "Well, how?" "He wipes it off with a cloth." Now, what is the duty of the physician in that case? There is a man who is probably spreading syphilis. Is it the duty of the physician to do anything in that case? Is his duty ended when he tells the patient that he should not continue that practice? Because to tell him that he should not continue to do so means for him to give up his livelihood.

Another case that sometimes occurs: A man is called upon to treat a man, also with a mucous patch. He finds him to be a man of family, with a wife and children. What is his duty? Is his duty ended when he tells the patient that he should not kiss his wife or kiss his children, and leave the responsibility with the patient himself? These are actual cases?

A third case: A man who is suffering from syphilis consults a physician. His hands are covered with syphilitic eruptions. That man is a baker, who deals out constantly from his bakeshop, bread and cake to customers. Is the doctor's duty ended when he tells him he should not do it? These are questions that we are all called upon to determine sometimes.

Mr. Backus (interposing): Dr. Raymond, may I say this? I would regard it as a pleasure at any time to answer any communication or question that comes up in your Society. If you will frame it and bring it to the District Attorney's office, either I or one of my assistants will examine the subject and try to answer it with as good authority as we can; and I regard myself as paid by you people as your counsel to answer those conundrums. I should be glad to see any of the doctors at the office at any time.

Dr. Raymond: A member of the Society has just passed up this question: "Should a doctor disclose the facts of this case? He finds a condition in a patient which demands abortion, which he performs, and the case results fatally." If I understand the law, there is a provision which protects the doctor who performs abortion for the preservation of the life of the mother or the child. The answer to the question, then, would be negative.—*The Brooklyn Medical Journal.*

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ABSTRACTS.

THE ACTION OF PHYTOLINE IN OBESITY.

I. N. LOVE, M.D., St. Louis,

Ex-President Mississippi Valley Medical Association; Ex-President American Medical Editors' Association.

The statement will be accepted by all, that fat is not the highest grade of tissue, its object in the anatomy being really as a packing material, externally serving for the artistic purpose of rounding out the lines of the figure in a manner to favor curves and that which is graceful, rather than angels and abrupt prominences.

Fat in proper quantity favors comfort and favors the really beautiful, and smoothes out the tangled threads of care and the wrinkled lines left by carking worries and the fuming and fretting of the every-day annoyances of life. It does not exist very much as a necessity to the physique, but rather as an accessory. That there can be too much of a good thing goes without saying, and the numerous mountains of flesh moving about our streets and hiding themselves in their homes will give evidence in favor of the truth of this statement.

An excessive amount of this fat is not only unsightly, but is unhealthy; in fact, as an evidence favoring the thought that fat is a low-grade tissue, we speak of other tissues degenerating into fat. Certainly the tendency towards the accumulation of an extra and unnecessary amount of fat favors a fatty degeneration of the heart and the tissue forming other important organs.

The proper selection of diet, with exercise, can no doubt do much towards the diminishment of fat; but the profession and the laity have long looked for some remedy which could be depended upon to assist towards this consummation devoutly to be wished. In Phytoline we have such a remedy, this remedy being prepared from the active principle of the Berries of the Phytolacca Decandra, after having been touched by the early frost. The remedy has been used for rheumatism for many years, and also for the diminishment of excessive glandular growths, and also as a helper for the drying up and the reducing in size of breast of nursing women. Inflammation and abscess of the mammary glands have often been prevented by its use.

The reason for its acting favorably in these conditions have not been given. Clinically the results have been satisfactory. In a number of cases wherin I have directed the using of the Phytoline for the reduction of fat I have had favorable results, and I am persuaded that the profession is justified in the use of the medicament. I have never had any pernicious effects following the use of Phytoline, and I believe it to be a perfectly safe and a harmless though a powerful antifat. I shall continue to watch results with interest.

THE TREATMENT OF SYPHILIS.

L. Bolton Bangs, M.D., Visiting Genito-Urinary Surgeon St. Mark's Hospital, in discussing the administration of mercury, says:

"The criteria at present for the administration of mercury are to be found in the disappearance of the pathological accumulations of the lesions which we can see and feel, viz., the glandular enlargements, syphilodermata, etc., and in the maintenance of the individual's general well-being.

"As to methods of administration: if the remedy is well borne when taken by the mouth, this is the preferable mode. Only so much should be administered daily as can be digested easily and freely without interfering with stomach and intestinal digestion, and only up to the point of the drug's physiological action. In the majority of cases an habituation to the remedy ensues, and, therefore, I am accustomed to make intermissions in the treatment from time to time, always, however, maintaining the hygienic and dietetic status quo of tne person. It should be observed that many persons do not bear the internal administration of mercury up to the point that may be necessary to act vigorously upon their lesions. In such cases the method of inunction should be resorted to at once. This is less depressing than the vaporization method, is not at all painful, and, in spite of the staining of the skin, is less disagreeable than the hypodermic method, and is often brilliant in its results.

"It is my custom, during the course of the treatment of syphilis, to intermit the internal administration of the drug, even if it is well borne by the stomach, and substitute inunctions, always keeping in view the general principles upon which the drug is administered.

"In certain malignant forms of the disease, where the tissues of the individual seem to be rapidly breaking down, hypodermatic injections of the bichloride may be given with very prompt effect. They should be repeated with care, bearing in mind the fact that death has ensued from this method of using the drug. With ordinary precautions, however, and choosing the buttocks for the site of the injections, there is, in my opinion, but little danger. It certainly is a very rapid and effective method of getting the system of the individual under the influence of the drug, but the injections should not be repeated too often. For example, I have seen a gentleman suffering from a very mild attack of syphilis almost exsanguinated by receiving an injection of the bichloride every other day up to the number of fifty.

"Even in cases of malignant syphilis the effect of the injections should be watched very closely, and every possible accessory measure taken to increase the general well-being of the patient.

"One of the most important guides to indicate the period during which treatment should be carried on is the condition of the glands throughout the body; particularly the epitrochlear and those which are not usually found enlarged in the more common dyscrasia. So long as these glands remain enlarged, it is one indication at least that treatment should not be discontinued. Enlargement of the glands alone is not positive evidence of syphilis, but if the practitioner has followed his patient from the onset of the disease, and has appreciated that these enlargements have grown smaller or disappeared entirely under treatment, it is confirmatory evidence that they are syphilitic in their nature. If the individual gives a suspicious history, and has glandular enlargements, it is well to give him the benefit of the doubt and administer mercury; but at the end of three months, say, if no material change has taken place in the size of the glands, it may be reasonably concluded that connective tissue changes have occurred in them to such a degree that they are now permanently enlarged.

"The duration of the treatment, irrespective of the state of the glandular system, will depend somewhat upon the individual and the continued activity of his disease. But experience teaches that long-continued and gentle treatment for at least two years (some authorities say five, and one has lately urged seven) affords the patient protection from the effects of the disease later in life."—*Medical Review of Reviews.*

PROCEEDINGS OF SOCIETIES.

THE CINCINNATI OBSTETRICAL SOCIETY.

April 8, 1897.

Treatment of Displacements.—Dr. Wenning: Mr. President, I spoke at some length last evening on the subject of pessaries, but it occurred to me, while Dr. Johnstone was speaking, that in some cases of acute retroversion particular attention must be paid to the immediate care of retroversion in the form of establishing a correct condition. Most of the cases we meet, of course, have existed for a long time, but there is such a thing as an acute displacement of the uterus. A young lady in one of the suburbs whilst romping fell over a chair and hurt her side. She screamed with pain. The first thing the physician did was to give her a hypodermic injection of morphine, which relieved her over night. The next morning the pain returned and he made another hypodermic injection, and so on for weeks and weeks. The patient became worse and worse and became thoroughly debilitated and a morphine debauche. They finally brought her to the Saint Mary's Hospital, thinking if she did not get well she might as well die there as anywhere else. I found, on examination, the uterus was retroverted, the fundus dipping almost down to the sacrum and firmly adherent. I began the treatment by massage and daily introduction of tampons in a Sim's or knee-elbow position, and finally succeeded in righting the position of the uterus. The patient expressed relief at once, from that time on. But the hardest thing to get rid of was the morphine habit. The body was covered from head to foot with sores, probably due to the use of a dirty needle. She told me when one particular portion of the body was peppered as much as possible, another would be used. After a few months, when she got over the morphine habit, she so improved that her friends hardly knew her. This case impressed me that in every case, no matter how young, an examination ought to be made.

Dr. Hall: Mr. President, I was very much interested in the remarks of Dr. Johnstone and the sanguine manner in which he says he cures 20 per cent. of these old chronic cases by curettage. That is about 19.9 per cent. more than I have ever been able to cure.

Dr. Johnstone: Mr. President, evidently the gentleman was not listening. I said most of them were in young girls; they could not be very chronic.

Dr. Hall: I stand corrected, but I understood the Doctor to say in young girls of long standing. I will grant we have better results in young girls, but in the old chronic cases I have not had very satisfactory results in curing them with the curette. Vaginal fixation for retroversion I discussed at the last meeting and I will not refer to it here.

But I will refer to Dr. Jones' paper. I believe the Alexander operation has had its best days. I believe, as expressed by one of the speakers to-night, we have fads in the profession, not only in medicine but in surgery, and I believe the fad of the Alexander operation is passing. It does relieve the retroversion. It is an ideal operation in many respects, but in very many instances the patient is worse off with a retroversion cured with a pair of hernias than she was before, and this cannot be gainsaid by anybody. Even Edebohls will have his hernia all right, and so will anybody who opens up these canals, especially in fleshly subjects. I have gotten tired of jumping out of the frying pan into the fire, Mr. President. It is very unpleasant to operate on a retroversion and have the patient come back to have the hernia cured, and then return the next year to have the hernia cured again. I believe we are asked for relief by the working class because they are obliged to be on their feet and need relief more, while the society person may be made more bearable without operation. But occasionally even wealthy people will ask for relief from great pain. Some people suffer much more than others from a retroverted uterus. Why this is true it is difficult to explain.

Discussion of Cure of Displacements.—Dr. Giles Mitchell: Mr. President, I will not take up any of the time of the Society, for at the last meeting I spoke on the question.

I am surprised that the speaker of the evening has had such a large experience with retroversion in young girls. Nearly all the retroversions that have come under my observation occurred in multiparæ, women who have borne children, women who have had cervical tears and perineal lacerations. Of course, I can appreciate how cases can occur in virgins from great violence, such as has been related by the speaker. But it seems to me this malposition is comparatively rare in virgins. I can also readily appreciate the value of curettage, but not to the extent of curing an ordinary retroversion. Of course, this is part of the treatment.

Now, as to the Alexander operation. I simply want to emphasize what I said at the last meeting. I am quite sure this is the ideal operation; this is the operation which is most feasible, sensible and scientific, and this is the operation which will last. I am astonished at the gentlemen who said this operation is passing away. The Kelly operation has been found fault with of late, and it is an operation which will be done less and less, and for good reasons. And the Mackenroth operation, also, I am quite sure will be done much less frequently by the gentleman the next year. If we are to take the statements of such men as Kellogg and Edebohls, who have had very few cases of hernia; when a man operates a thousand times and has very few hernias, it seems to me the danger from this source is comparatively slight. There is no question Kellogg is a credible witness; Edebohls quotes him in his recent article, and gentlemen have gone there to see him, and I have no doubt his statements are correct. If the operation is done after the method of Kellogg, as quoted by Dr. Jones, or after the method of Edebohls, or any of the approved plans, hernia ought to occur very seldom. It seems to me, from the statements I have read, the greatest danger is from rupture of the round ligament itself. Of course, no man would think of trying to repose the uterus where there were numerous adhesions until the adhesions were gotten rid of, and they are gotten rid of by a long plan of treatment, such as vaginal suppositories of glycerin. And I am very sure the Alexander operation is the one we will all be championing in the next fifteen or twenty years.

Treatment of Displacements Uteri.—Dr. Bonnifield: Gentlemen, I, like most of the other gentlemen, spoke on this subject at the last meeting. I now merely want to refer to one or two things. My experience is not like that of Dr. Johnstone's, in regard to breaking up the adhesions. If a case comes to me with a uterus retroflexed or retroverted (they are usually retroflexed), if the adhesions are very extensive, and under an anæsthetic I cannot discover any disease, I do not hesitate to break up the adhesions by the Schultz method. The fact the Doctor speaks of is undoubtedly true, that hospital cases, cases of working girls, etc., are those that most imperatively demand operative procedure; just as a man doing light work may get along with a truss, but a street laborer has a pretty hard time, so a person in moderate circumstances may get along with a pessary, but the girl who works, as with a sewing machine, must have the condition relieved. I thoroughly agree with Dr. Johnstone in curetting these cases. I am not prepared to say whether it is 20 per cent. or more that I have cured with the curette and other means. The use of the curette is the first step and one of the most important ones.

Treatment of Displacements.—Dr. Edwin Ricketts: Mr. President, in regard to the use of pessaries I do not want to go over that ground; you know my views. As to the operation of Alexander, the men abroad, who are associated with him, are the ones to throw this operation down. The leading men doing gynæcological work, who knew of the Alexander operation when it first came forward, are the ones that say, as has been said here to-night, that it is not the operation to be preferred, except in a very few cases. And as to the gentleman on the left making the assertion that the operation of Vineberg being relegated to the past, I beg to differ from him. I would beg to ask the gentleman if he has ever seen the operation done.

Dr. Giles Mitchell: I never have.

Dr. Ricketts: Then I do not think that is a fair criticism to make. The cases I have seen operated upon, while it is as yet too soon to pass final judgment, I want to say that they are simply superb in character. I don't care who may denounce it, I have myself seen very good results of the work. We know the retroflexions give us the greatest trouble, and we all know foreign bodies in any part of the body can give a great deal of trouble, and, as stated by Dr. Jones, a pessary can be the cause even of cancer. And in the operation of Vineberg you do not have the danger of hernia, as you do even in the operation of Alexander, itself.

Dr. Mitchell: The reason I have not felt that the operation would stand is because of the various criticisms made. The men who have followed the operation have abandoned it. It seems to me the operation should be abandoned.

Dr. Hall: I would remind the gentleman that the cases he refers to are not those made by the Vineberg operation at all, but those in which the uterus was stitched to the vaginal wall, and not fixed by the round ligaments, as in the Vineberg operation.

Uterine Displacements.—Dr. A. W. Johnstone:—Mr. President, I am a little surprised to learn from some of the speakers that they do not see any retroversions in young girls. I think I see almost as many in them as in others. I have two on hand now, one æt. 16 years and the other 22. You have no idea how common this is unless you happen to have a run of those girls. The finding of a retroversion in that class of cases I believe is the saving of a lifelong trouble, because you can cure that class of cases much more easily than you can in the multiparæ.

As for the vaginal fixation, I would like to ask if any of the gentlemen have had a sort of cellulitis after the operation, a persistence of the inflammation around the scar, that comes and goes with each little attack of cold and makes the whole vagina hard. I want to know if that is an unique experience.

Dr. Hall: I have not had any like experience; it is a new case to me.

Dr. Johnstone: I would like to just detail a case I had once. It appeared as if it would be a hard case, but to my surprise I found not a single adhesion, but the broad ligaments were shortened up in a peculiar way. In those days I was perhaps more daring than I am now, and I pulled the uterus up and found it to fall backwards. And I have often thought, when you get hold of a uterus you cannot tell whether it is fixed to the vagina or not; these appendages may twist the uterus back and hold it there, so it is a deceptive condition.

Discussion of Uterine Displacements.—Dr. Bonnifield: In regard to the adhesions, that is very true. Oftentimes the uterus is sharply retroflexed; but in those cases, if one is sufficiently skilful—if he will carry two fingers in the vagina, even with the patient on the back, if the abdomen is tolerably lax—he can throw the uterus back, or possibly it may be necessary to turn the patient on the back, but it requires considerable force. The adhesions I speak of you can feel, and you can feel them give way. You cannot put the uterus in position until you have pulled it down with a vulcellum and done away with the adhesions, and then you can replace the uterus. In those cases, of course, you cannot always tell whether the appendages are diseased or not, and if I had any doubt I certainly would not resort to this method. When we fix the uterus forward, we simply substitute one pathological condition for another, and it certainly will require one or two years before we can determine the value of the procedure

Three Cases of Appendicitis.—Dr. Edwin Ricketts: Mr. President, this is an appendix removed from our friend, Dr. M., three weeks ago to-day. He gives a history of returning appendicitis for ten years. He had what he called a blind hernia. He was out of his work for a year. Previous to the operation he was in bed for two days. The distal end of the appendix was found attached to Poupart's ligament. There were present two or three drops of pus. We so often hear the cry: Wait for pus! Wait for pus! Yet for pain we were justified in the operation in this case. There has been no trouble whatever in the recovery of the Doctor.

I want you to notice the distal end of this appendix, which was removed from a woman aet. 50 years, who had recurrent attacks of appendicitis for eight years. She came into the hospital in a stooping posture, which position she had maintained for a year. The diagnosis of appendicitis was made and the abdomen was opened a week ago to-morrow. A strange state of things was found. This appendix was five inches in length, and the distal end was attached to the ovary and fallopian tube on the right side.

The temperature has not been above 98.5°, and I never saw a patient do nicer.

A girl, 14 years of age, was taken ill on Thanksgiving Day, up in the State, although she was a patient of Dr. McGrew and resided in Pleasant

Ridge. The Doctor was called to see her and found her with fever, 99.5°, he told me, and her pulse was 110. A few days after coming under observation she was taken with dyspnœa. I reached her on Sunday morning early, and at that time the distress of this patient was something frightful to behold; her face was livid and her respiration gasping, so I was satisfied we had pus in the pleural cavity, and pushed in a hypodermic needle and withdrew quite readily a syringeful of fluid. We immediately gave her chloroform and cut into the cavity and took out about a pint of fluid. She had blisters over the region of the appendix. Within three days we were able to take the gauze out of the pleural cavity, and, strange to say, we let it heal and we have had no trouble with the pleural cavity. Two weeks, lacking two days, we opened up the abdomen and turned out almost two quarts of pus—it seems almost incredulous to relate and yet it is true—and the most foul-smelling pus, which is so characteristic of appendical abscesses. The gauze was made use of in drainage and now the patient's temperature is 99° and the pulse down to 90 and up to 100. She is eating and we have every indication of the patient making a nice recovery.

These cases I think about cover the ground of appendicitis.

This specimen was removed from a lady 50 years old. She first noticed this tumor last September. The diagnosis of ovarian tumor was made, and 48 hours ago at eleven o'clock this morning, the operation was done. On incising the sac we found the fluid would not run. It was my first experience with such fluid. On the opposite side we had an ovarian tumor. A double ovariotomy was done, and this patient now for about 55 hours has been doing well. She has not taken a dose of morphia. And in the case of appendicitis in a lady we got along without morphia.

The bladder in this girl was pulled up about to the umbilicus, and in incising the abdomen I laid the bladder open before I was aware that it was anywhere near, and before proceding with the operation I repaired the damage with silk worm gut, putting in six interrupted sutures, and I have not had any trouble with it. I was careful, after stitching the bladder, to close the one above so no infection could get in. There has not been any untoward symptom and it is now three weeks since the operation was done.

Dr. Bonnifield: Were there any papillary growths in the larger tumor?

Dr. Ricketts: No, sir; I would say the pedicle to the larger tumor is the largest I have ever put a ligature on.

Discussion of Dr. Rickett's Cases.—Dr. Johnstone: Mr. President, one of these cases, which referred to the pus in the pleural cavity, is a rather rare point, but one to which we do not pay enough attention, and that is the connection between the hepatic, diaphragmatic region and the appendix. The first time my attention was ever called to this was something like 12 or 14 years ago, after I had several times opened abscesses of the appendix. There was a little Jew dropped in a town down in Kentucky where I was practising. I treated him along for a week or ten days, when to my great surprise he developed what seemed to be acute hepatitis. The mass projected an inch or two below the ribs and was very sore. There was no literature on the subject at the time with which I was familiar.

In about a month the same patient was off with a case of croupous pneumonia, which filled all of the lower lobe of the right lung. I am happy to say the man got well. There was a progressive inflammation without suppuration, which traveled up the right side, gave an acute hepatitis and later a croupous pneumonia. Since that time I have had a case (last year) which was a sad thing all around. A leading lawyer of his region I was called to see and I found what seemed like a case of chronic appendicitis. I saw him about one o'clock in the morning and I had to come back on the three o'clock train. It was perfectly plain nothing could be done there; he would have to be brought to the city. In studying the case I got it into my head that it was not an appendicitis, but carcinoma. Without saying anything to a general surgeon of this city, with whom I consulted, he came to the same conclusion. The man would have very great colicky pains now and then. The lump was not larger than a walnut, just in the region of the appendix, although a little further out, nearer the flare of the ilium than you would expect an appendicitis. An exploratory incision was made and the little lump of carcinomatous tissue was found near the ileo-cæcal valve. We would have exsected it but for the fact that the glands were infected. The patient did beautifully for three days and then on came a catarrhal pneumonia. He had a markedly tubercular family history. He had been alarmed, himself, by the fear that he had tuberculosis. The whole question with me has been whether that was an old smouldering tuberculous condition of the lung or whether it was the ether that started it, or whether it was the connection of the lymphatics around the ligature and through the pleural cavity. It is known that some of the lymphatics pass in this way through the central tendon of the diaphragm to the pleural cavity. I am only too glad that Dr. Ricketts reported this case, because it is just in line with one over which I have had a good deal of thought. A case of subhepatic abscess is reported in one of the journals here in this city, which was produced in the same way.

Discussion of Dr. Ricketts' Cases.—Dr. Giles Mitchell: Mr. President, while Dr. Hall was reporting his case of fibroid tumor and giving the most cogent reasons for the removal of these growths, I thought of a case which

I operated on for a fibroid about three times as large as the one the Doctor exhibited, three weeks ago yesterday at the Presbyterian Hospital, where the history given was of five or six years' standing, where there had never been a history of hæmorrhage, where the menstruation had been regular, had never gone beyond five days, and the only discomfort the patient had was during the last year a sense of weight and heaviness about the pelvis and frequent attacks of nausea and vomiting. This is the only case I have encountered in which hæmorrhage was not a pronounced symptom. Of course, after a tumor becomes subperitoneal you do not have any disturbance of the menstrual function, but usually the hæmorrhage is pronounced and sometimes continues if the cases are not operated on soon enough.

Dr. Rufus B. Hall: Mr. President, I want to make a few remarks on the bladder injury of the case of Dr. Ricketts. I have injured the bladder under similar circumstances on more than one occasion, and I have never thought it of sufficient importance to say much about it. Injury of the bladder in abdominal operations I do not consider much of a calamity to the patient. I have never seen a case do badly after being injured in that way. The reason I brought this question up was that I was a little surprised at the Doctor's method of treating the case. I have always treated those cases as I would an ordinary cholecystotomy, attaching the bladder to the abdominal wall and passing the sutures through the abdominal wall and the bladder, so that the peritoneum of the bladder would be brought to the abdominal peritoneum, and then fix a little drainage tube through the abdominal wall into the bladder an inch or two for 48 hours, and during this time have the patient catheterized every three or four hours, or if necessary have a self-retaining catheter introduced. I remember one instance in which I laid the bladder open fully eight inches. I laid it open where it doubled over on to itself, through two thicknesses of the bladder. At the time I reported this case in detail, so I am not ashamed to report it again. I am not criticizing the Doctor for injuring the bladder, but I do not like to put a buried suture into the bladder for fear I may hear from it again.

Dr. Ray, of Portsmouth: Mr. President, I do not know that I have anything to say, except in connection with the cases of appendicitis, especially that of Dr. Ricketts' with suppuration. I had some experience with that class of cases last summer in opening up an abscess caused by appendicitis and afterwards drained for some length of time. The Doctor in charge allowed the man to get up, supposing he was well, for it had quit discharging, and two or three weeks afterwards I noticed in the paper that the man had died. I saw the Doctor afterwards and asked him what was the matter with the man. He said the patient got worse and they put him back to bed and he took pneumonia and died. At any rate, the patient died with a right-sided pneumonia. I believe, if I understand the Doctor right, in his treatment of the bladder he treats it as if he had done that on purpose—he made a suprapubic lithotomy.

Dr. Bonnifield: In regard to the pus traveling up, I reported a case of appendicitis, very acute in its incipiency, which I saw on Mt. Auburn with Dr. Forchheimer. In that case the boy convalesced quite satisfactorily for several weeks. At the end of that time I examined him very carefully and was unable to find anything in the region of the liver or anywhere else to account for the temperature. Suddenly the temperature began to get higher, and one day it shot up to 105°; Forchheimer and I were telephoned for to come there, and we found the boy spitting pus. He then recovered.

I saw a very large fever temperature only recently, in which the patient gave no history of hæmorrhage. The hæmorrhage comes from endometritis, and if we do not have hypertrophic endometritis, we may have hæmorrhage.

Dr. Giles Mitchell: I would just like to say that Dr. Illoway reported to this Society, about thirteen years ago, a case in which the pus from a pelvic abscess after a number of months perforated the diaphragm and the woman expectorated a large quantity of pus.

Adjourned.

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NEWS AND NOTES.

The ninth annual meeting of the Tri-State Medical Society, of Alabama, Georgia and Tennessee, will be held in the Senate Chamber of the State Capitol, Nashville, Tenn., Tuesday, Wednesday and Thursday, October 12, 13 and 14, 1897. If you desire to read a paper, report a case or exhibit a specimen, notify the secretary. Reduced railroad rates to the Tennessee Centennial. Frank Trester Smith, M.D., secretary, Chattanooga, Tenn.; W. F. Westmoreland, M.D., president, Atlanta, Ga.; W. D. Haggard, Jr., M.D., chairman Committee of Arrangements, Nashville, Tenn.

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EDITORIALS.

BACTERIOLOGY IN A GENERAL WAY, NECESSARY.

Our readers have so long been accustomed to seeing the large number of extracts and articles in our pages on bacteriological subjects, that they may think it a work of supererogation to refer to the necessity of a general knowledge of this branch of medicine: nor would we have done so, except to quote an excellent recent editorial from the Medical News, entitled "The Importance of a General Knowledge of Bacteriology." When a writer gives expression to ideas which we thoroughly endorse—does so in a way that is most commendable we are only too glad to quote the original text, and so we do in the present instance—"The discovery. etc., —— —— and prompt to obey."

The discovery of bacteria and their place in biology, the consequent complete revolution of many lines of thought, the abandonment of old theories and the establishment of new ones, have occurred so recently, and have followed each other with such rapidity, that much confusion exists in the public mind in regard to the subject. It is this confusion which prevents the accomplishment of the practical results which should ensue from this great acquisition to science. On the one hand there exists a morbid fear of the very name bacteria. on the other there is a skepticism quite as pernicious, and it may be said in passing that the unreliable statements of the newspapers are largely responsible for both beliefs. It is by means of our profession that confusion will be transformed into order. A clear knowledge of the life history of these important organisms will go far to rid the public mind of an unnecessary anxiety, as well as to indicate where the real danger lies, and it is the duty of every physician to disseminate practical information in regard to microbiology whenever practicable.

It should be generally known that bacteria are minute organisms belonging to the lowest botanic orders; that each consists of a single cell, so small that it must be magnified 400 or 500 times in order to be observed; that these plants have the power of increasing in numbers very rapidly under favorable conditions by simply dividing into two, each half becoming a fuly developed organism like the original; that these plants are, like their larger relatives, some useful and some harmful; that many of these plants have the power of taking in food and converting it into a poisonous substance, just as the deadly nightshade, planted in the same soil and inhaling the same air as its cousin, the potato, will convert its food into poison, while the latter is a most useful food; that the poisonous substances formed by the bacteria, when absorbed by the human economy, produce diseases, modified, among other things, by the intensity of the poison, the rapidity of its absorption, and the location of its inception.

From a strictly utilitarian standpoint we may roughly classify bacteria as (1) useful, (2) harmless or neutral, and (3) harmful. Judging from the sensational articles appearing from time to time in the newspapers, one would suppose that every germ is a disease-producer, and that death to all microbes is a result devoutly to be desired; but, as a matter of fact, the vast majority of bacteria (both as to species and numbers) are either harmless or positively beneficial, the destruction of which would be in the nature of a calamity. Perhaps the greatest benefit which we derive from these little germs is their role in the oxidation of organic matter. All dead vegetable and animal matter is acted upon by them, and the complex chemic substances of organic life are reduced to simpler compounds, thus rendering innocuous what might be dangerous, or at least unpleasant, and at the same time furnishing those elements which enrich the soil and make possible the harvest. It has been said that without microbes crops would be impossible (Duclaux).

Microchemistry is a prime factor in the cleansing of water. By its action drinking-water which is contaminated with organic matter is rendered perfectly pure by the conversion of dangerous chemic compounds into harmless nitrites and nitrates; it causes the ripening of cheese; it produces the fine flavors of butter as well as some which are not so fine, and it is probable that at times bacteria are an aid in digestion of food. At any rate, there are 331 described species of bacteria which are useful, or at least harmless, and only 158 species which are known to produce disease, and comparatively few of these affect the human family (Sternberg). And, moreover, since the conditions for existence of the disease-producing germs are not abundantly supplied, they have, on the whole, a rather hard life-struggle, and it is not wonderful that when the opportunity presents they improve it to the utmost.

It is the harmful germ with which we are most concerned. The useful germ we accept, like air and sunlight, as a matter of course. The harmful germs, after evading every effort to discover them for centuries, are rapidly being isolated. studied, and the influences which are inimical to their continued existence established. It was found that certain diseases, of which typhus is a type, depended for their propagation on the presence of filth. Measures were taken to remove the filth, and typhus disapppeared. It is known that typhoid fever, malaria, cholera, and certain diarrheal affections are produced by the presence of germs in drinking-water, and that if such water is boiled these diseases will be prevented. It is known that the sputum of a tuberculous patient, when allowed to dry, may infect others, and that if the sputum is burned the major danger is avoided. It is known that air-borne diseases are, as a rule, carried but a short distance, and that if a patient suffering from them be isolated, and if all discharges and articles of clothing, etc., with which the patient has been in contact are disinfected, the danger of others contracting the disease is comparatively slight. It is known that certain diseases are prevented or modified by vaccination or inoculation; that this acquired immunity is produced by a well understood scientific principle, which is being repeatedly proven. It is known that for pus microbes, as well as others, there is nothing so conducive to their growth as heat and moisture, and that an old-fashioned poultice supplies these conditions most admirably, and that in the discarding of this dangerous agent for the treatment of open wounds, whether infected or not, and in purulent affections generally, much suffering and perhaps some lives have been saved.

These precepts are thoroughly practical, and even a superficial knowledge of the principles of bacteriology will convince the public of their importance, and the necessity of better measures of quarantine; of the enactment and enforcement of laws in regard to isolation and vaccination; of disinfection; of general sanitary cleanliness, including pure food, pure milk, pure water suppply, and pure air; matters in which every individual is vitally interested. The more thoroughly bacteria and their effects are understood, the less we will hear of the scoffer, the anti-vaccination crank, and the mob which resists the health officer in the performance of his duty.

The public is beginning to see that the present practice of medicine is founded on science, and it is anxious to know more of it. It is remarkable what an interest people are taking in this scientific basis of medicine. Whether due to the training in science and physiology now common in our public schools or to some other reason would be difficult to decide, but certain it is that there is a wholesome desire for information which the physician should be the last to withhold. He may communicate the fact to his patients that it is unwise to poultice a boil; that it is necessary to disinfect polluted clothing and diarrheal discharges; that it is desirable to boil suspicious water, etc. Let him also explain to them why these things should be done, and in the majority of cases he will find his audience only too glad to listen, and prompt to obey.

A MEDICAL EDUCATION.

Upon the eve of a new year in medical teaching, it cannot be amiss to revert to a question which we have had occasion to touch upon before—the preparation of a man for the practice of medicine. We have always maintained that medicine should primarily be regarded as a calling, not as a means to gain a livelihood. In this respect it ought to be upon the same footing as the ministry. It reminds us of the selection of a companion for life. The question is not who can you live with, but who do you find it impossible to live without. So with medicine: to attain the best results, to be the true physician, a man must feel that he would not be content to do anything else; it must be a necessity to complete his happiness. If it is irksome, if he seeks to shun the ordinary or extraordinary demands of his calling, if he does not follow with the same interest a case that is unable to pay for his services as his most remunerative private patient, he can never expect to reap the full benefit or pleasure which the practice of his profession affords. He must recognize that his duties to his patients take precedence over any and all others. Social pleasures and social engagements must always be made with the understanding that they are subject to the demands of his professional duties. If then the embryo physician enters upon his calling in this spirit, the question of a medical education becomes one not difficult of solution. It goes without saying that a general education of the best character is none too good for such a responsible profession. That a man may be more or less successful without it by no means argues against its desirability and necessity; for those who have, without the benefits of a good, broad, general education, attained eminence in medicine, must continually reflect that they could have done much more and done it with greater intelligence and less effort, if they had received the proper foundation. The purely medical part will be determined by the locality, cost, time and numerous other accidental considerations. The student may with benefit reflect, that if he applies himself diligently he can complete a good medical course in three years, and then with profit spend two more in hospital work. If his means be abundant, he can spend one or two additional years in college work, and instead of "walking" on hospital, serve as interne in a general hospital and in an institution devoted to special work. Of course, it is not possible for every graduate to get desirable hospital work; but it is eminently proper to make the effort, and not to give up until something has been attained in that line. It is only after a man has been in practice some years that he realizes at what a disadvantage a man works who has had no hospital experience. At the end of this time it may be said that a man has been prepared for work in medicine; and this does not mean that there is nothing left for him to learn, but that he has just reached the point where he can make the most of the opportunities daily offered to add to his store of information. He will compare the methods of work followed by different men and do so intelligently; and he will be better able to elect what is good and serviceable. It is one of the rarest and at the same time one of the most valuable accomplishments to be able from the great mass of good, bad and indifferent propositions weekly offered the profession, to select that which has real merit and which contains the germs of useful and practical worth.

We welcome to the study of medicine those who enter upon it with the proper understanding of its principles and responsibilities, and who feel that no other work will enable them to fulfil their mission in life.

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ORIGINAL ARTICLES.

SOME OF THE CAUSES DEFEATING THE PROPER PROGRESS OF THERAPEUTICS.*

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In the consideration of this subject, the question naturally arises as to whether or not there is a progress in therapeutics which, when compared with the advance in other departments of our art and science, can properly be considered commensurate. The answer to such an inquiry can be most briefly and positively stated, and, with regret, is formulated in the negative, by a perusal of the latest standard works of reference. With few exceptions those diseases with which we are most frequently confronted, and which comprise the more common or prevalent, compel the realization of the fact that therapeutics cannot lay claim to much. when viewed from the standpoint of the physiologist, pathologist and surgeon.

Let us consider such a frequently encountered affection as acute croupous or lobar pneumonia. Its treatment to-day is still argued as it was decades ago, and the pages of volumes of reference, as well as the fiat from the professoral desk, teem with interrogations that should long ago have been definitely and positively settled had the same careful and analytic study been instituted that characterizes research in other channels. Etiologically the almost exclusively mechanical interpretation of the circulatory function of the affected pulmonary lobe, if this is the only area diseased, which is submitted as an explanation for that peculiar exudate which it may here be remarked could not be caused by any interference, purely mechanical, with the venous or arterial side in loco—still obtains, and the advocates and adversaries of phlebotomy and the champions of aconite, tartar-emetic and veratrum viride all have disciples, who find the subject completely unsolved by the tests of practice as it was a century ago. The average mortality of twenty-eight per cent., now as then, substantiates the fact that in this disease therapeutics has not progressed.

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* Read before the Philadelphia County Medical Society, June 23, 1897.

Why, in the light of the facts, are these incomplete teachings still perpetuated? The great frequency of this disease and the opportunities offered for lines of thought and investigation in channels dissociated from the traditional beliefs, when still surrounded by this circulatory infatuation, in spite of its almost incessantly demonstrated uselessness, should, at least, direct attention to the study of its treatment upon an entirely different basis. If there is one fact established about pneumonia, it certainly is that it is not, per se, an inflammation of the lung. It is unquestionably an expression of a trophic derangement, followed, it is true, secondarily, by inflammatory phenomena, and why therefore the early treatment should still center about the antiphologistic notion is a question that can fittingly be propounded.

Syphilis will also serve as an illustration; its natural history, so to speak affords a clinical picture so distinct that, even though its materies morbi is still in doubt, the ultimate outcome is comparatively easily prognosticated, and yet its therapeusis is still debated around the mercurial and non-mercurial methods and such sub-divisions thereof as the interrupted and continuous, mixed, and several other useless combinations and modifications, which, when carefully followed as to their results, discloses negatives that should long ago have relegated them to the oblivion they so well deserve. There is scarcely any affection which, if skilfully treated according to the rules that should be determined by the conditions obtaining in the individual sufferer, offers such promise of relief as this disease, and when the notions that are formulated in the terms already indicated will have been abandoned and a proper advance made in its therapeusis, the psoriasis lingualis syphilitica and other commonly seen lesions of the mucous membrance will cease to be so frequent in cases that have been discharged as cured but improperly treated as the result of these modified plans.

Pertussis, notwithstanding our ignorance of Afanassiew's bacillus, has recommended for its treatment, in the latest works issued by a prolific medical press, a host of pharmaceutic preparations, the trial of which long ago incontestably proved their impotence, and yet there is perpetuated, to the discredit of the most important department of our profession, such teaching.

If the treatment of the acute inflammatory troubles most commonly met is examined, what do we find? In simple acute laryngitis, quinia is advised in quantities for which a caution is deemed necessary in order not to add to the existing trouble aural and cerebral difficulties that would be decidedly worse than the affection to be treated. Are such absurdities unusual? Unfortunately they can be enumerated for too many diseases that long, ere this, should have ceased to be so managed.

As to the therapeutics of simple acute pleurisy, it suffices to point to the many instances of irreparably disabled victims, whose health has been damaged by a serious deformity resulting from changes of structure requiring an ultimate resort to surgery, necessitated by the consequences of a plan of treatment which advance in bacteriology and pathology has demonstrated to be no longer valid, and to rely upon applications, and the exhibition of absorbents which, over and over again have been proved to be almost useless, until an inocuous transudation has become the seat of retrograde changes, resulting in the development of a focus for infection and destruction.

Well may the injunction, intimated in one of our most recent and reliable works of reference, be seriously contemplated, with a view of establishing treatment at least different from that which has and still prevails: "That is medicatrix naturæ" is probably the chief remedial agent in many cases, non-rheumatic, in which (latter) cure takes place under the use of the salicylic compounds, potassium iodide or other specific drugs. It is better ofttimes to do nothing, in so far as remedial agents are concerned, than to institute procedures about which, if anything is known, the least and most that can be said is that no appreciable effects have been secured. To apparently be doing something and having as a result nothing, lends to nothing a dignity and value dangerous to the integrity of the profession!

It is needless to further consume time with additional illustrations, and attention is directed to some of the causes operative in the maintenance of the situation just depicted. The most important factor doubtless is a lack of knowledge on the part of the profession of the natural course and termination of disease. Very meager are the published facts governing this important theme, and teaching is conspicuous for the absence thereof, and consequently the clinician, no matter what plan of treatment is instituted, is incapable of recognizing whether or not morbid processes have been modified beneficially or injuriously.

Reflection upon this fact discovers much to account for the perpetuation of relatively valueless therapeusis. Dependence upon the goddess of fortune too commonly pacifies conscience and engenders a habit of thought and procedure pernicious to therapeutic progress. To such, at the bedside, the exhibition of almost anything, alone or in complex combination, suffices so long as the patient continues to improve, and when this does not take place there is a consultation or a change of physicians resulting in a resort to some ridiculous "pathy" and such careless and defective therapeusis is crowned with a result just as good, or, more correctly speaking, just as bad from one plan as the other.

Another cause is our materia medica. Why the brain of a student must be engaged at the expense of energy and time that should be occupied in the acquisition of more valuable knowledge in memorizing the name, habitat, natural order, preparations and doses of a lot of obsolete and useless drugs is a question not inopportune. Such substances as castorum, camphoric acid, lactucarium, veratria, urethan, oxalic acid, geranium and an array of innumerable and superfluous preparations of even our standard drugs, should be relegated to regions remote and not be a cause for an anxiety in the green room, which, unfortunately, does not end there, but is in danger of inculcating a lack of confidence in medicines of value and unquestioned merit, frequently reaching far into later life.

Associated herewith is the congener pharmacy. The classification of drugs is based upon their especial and intrinsic or inherent properties, and as many are derived from the vegetable kingdom, in which according to conditions there existing, they must of necessity be characterized by complexity of composition and we have variable and uncertain means. Several active principles are contained in one crude drug, and according to the solubility of these in the medium employed in pharmacy do tinctures, infusions, decoctions, extracts alcoholic and aqueous, and the drug itself represent remedial agents from which it is impossible to obtain uniform, definite or specific results, and to look under these conditions for achievements in therapeutics which should be distinct and unmistakable is unreasonable.

Again, even the chief active principle, or that which classifies a drug, is present in such a varying percentage that it is impossible to prescribe any of these prepations, from this or that pharmacy, with anything like precision and accuracy. Among several remedies, I have investigated the tincture and fluid and solid extracts of aconite root, and repeatedly found such variation in therapeutic power as to render it impossible to regulate the dose with the hope of securing anything like definite results. One tincture, by carefully graded increasing quantities was found to be inert so far as physiologic phenomena were concerned until administered in doses of one fluid dram. Is it any wonder, then, that these conditions which obtain alike for all drugs thus derived, result in the abandonment of many most valuable remedies, and a consequent misinterpretation, not only of the virtue of remedial agents, but also of the modification of the natural course of disease, and consequently of therapeusis.

Our preparations should be assayed not chemically only, but both chemically and physiologically, for to secure a uniform standard of strength for organic alkaloidal derivatives by chemistry alone is, for obvious reasons, impracticable; hence the necessity for the physiologic. Another point, in a sense foreign to the profession, but no small factor in obstructing progress, is the unreliable pharmacist, but as this is somewhat beyond the subject-matter of these remarks, it will be dismissed with a passing notice.

Thus obtain conditions within our province to correct which determine the existence of a now well-established custom, alike destructive to the integrity of clinical medicine, therapeutic progress and the art and science of pharmacy. Judging from the enormous manufacture and consumption of the many varieties of foods and the falsely designated diastasic products, the inference is fair that too commonly does the profession seek guidance for therapeutics from the pamphlet literature of this modern but evil institution. A diagrammatic illustration of an alleged microscopic field exactly demonstrates how a red blood-corpuscle opens its receptive channels at once to the chalybeate which, manufactured here or there, is dubbed a knight of peculiar and mysterious power.

Again, our trusted friend, cod-liver oil, which we know effects its truly beneficial influence upon metabolism when administered complete, is presented to us in such fragmentary and associated conditions, each one of which, according to this class of literature, is said to embody the virtues of its entirety as to largely result in the defeat of its purpose; and what has been said of this is applicable equally to many more of our medicines. The avidity with which the clinician grasps this is another illustration of an obstructive factor already mentioned.

The compressed disc is another evil and should be accorded a place befitting its true value. That such products render impracticable proper dosage and combination, and therefore defeat skilful and effective therapeusis, is obvious. A sadder sight, conspicuously sitting as judge over such customs cannot well be conjectured than that of the slowly and struggling convalescent being improperly nourished by one of these many products composed of cheap whisky, well diluted, mingled perhaps with a pretense of some proteid element and flavored with an essential oil and licorice; and yet it constitutes a type of that degenerate therapeusis which already has in a great measure supplanted the legitimate and taken the place of that well-trained, skilful and scientific practice that should be the purpose and highest achievement of the profession.

COMPLETE BLINDNESS DUE TO POISONING WITH JAMAICA GINGER.

ARCHIBALD G. THOMSON, M.D., Philadelphia.

The case about to be reported presents several interesting points which may throw light upon or help to confirm some of our ideas on the subject of toxic amblyopias.

J. R., a sailor, 32 years, related that his mother and father were both dead, from causes to him unknown. Had had the diseases of childhood, but had never suffered from any serious disease. He denied absolutely any venereal disease. He had never been a steady drinker, but from his occupation as a sailor would abstain from alcohol for several months at a time, on reaching shore going off on a spree for a couple of days two or three times in the course of a year. He had been moderate in the use of tobacco, smoking weekly about four ounces in a pipe. His eyesight had always been good until about December 22, 1896, when he came ashore from a cruise, and, with a party of friends, being unable to procure whisky, got intoxicated with Jamaica ginger of the ordinary commercial kind sold in small country stores. This was taken, as whisky is, with water. The man remained drunk for two days, estimating that during that time he took about a quart and a half of Jamaica ginger. On the following day the man's feelings were similar to those that ordinarily follow an alcoholic debauch, though greatly intensified. Among other things, he suffered from headache, nausea and vomiting.

On the evening of the next day, the fourth from the first taking of the ginger, while lighting the lamp in the cabin, the man noticed everything to be hazy and vision failing, together with some photophobia.

By the next morning he could not see a lighted match held directly before his eyes, but had peripheral vision sufficient for him to grope around. By the following morning, however, light-perception had absolutely disappeared, both central and peripheral, and blindness was complete. This condition lasted for seven days, when vision began gradually to return, first in the periphery of the field, the man finally being able to read large print with great difficulty.

The time that elapsed between the period of complete blindness and the recovery of vision the patient estimated at about four weeks. Matters remained thus stationary for three weeks, when vision again began to fail, now very slowly, until the present condition has been reached, three months and a half after taking the Jamaica ginger.

The anterior aspect of the eye is apparently normal, cornea and conjunctiva. The pupils are slightly dilated, but react both to light and in accommodation.

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* Read before the Philadelphia County Medical Society, June 23, 1897.

V = R. Fingers at 1 meter.
L. 1-100, eccentric.

On ophthalmoscopic examination the media are found to be clear. The discs are exceedingly pale, the capillaries being almost entirely absent. The portion of the discs occupied by the papulo-macular bundle of fibers (forming the lower and outer quadrant) is completely atrophied and greenish-white in color.

The fundus of each eye is otherwise normal. There is a well-marked physiological cup and the absence of lymph from the vessels, showing the atrophy to be primary and not secondary to a previous papillitis.

There is no evidence of cerebral or spinal trouble (no locomotor ataxia or disseminated sclerosis, or hysteria.

The patient states that one of his friends, who accompanied him on his debauch, has also marked disturbance of vision, though in less degree. Under treatment this entirely disappeared.

On questioning the man more closely, some three months after I first saw him, he stated that since his visual loss he has heard of several other cases in which the sight was affected by drinking Jamaica ginger, and that the practice of using this as a beverage is quite common in districts where it is difficult to procure alcohol.

The case presents thus several very interesting features:

1. The uncommon nature of the agent producing the poisoning.
2. The suddenness of the attack of complete blindness.
3. The peculiar changes in the fields which correspond most probably with the pathologic condition that takes place in the nerve.

There is a difference of opinion between authorities as to whether alcohol adulterated with certain substances is more or less liable to produce toxic amblyopia. However this may be, I think it is generally conceded by all that the cheapest and worst kinds of alcohol are more likely to bring about this condition.

I have been unable to procure a specimen of the kind of Jamaica ginger taken by the patient, which he describes, however, as being the ordinary commercial kind; so I wrote Professor Remington, of the Philadelphia College of Pharmacy, asking if he could inform me as to the ingredients contained. To my inquiries I received the following courteous reply:

"What is known as Jamaica ginger, or, more properly, essence of Jamaica ginger, is made in a very simple manner, by percolating the ground ginger-root with alcohol or alcohol containing water. The cheap trash found in country stores is often made with a portion of capsicum, in this way saving expense. You can see that a very little capsicum would take the place in pungency of a large quantity of ginger. Of course sufficient ginger would have to be used to give a flavor to the decoction. Then, again, it is made hot with cayenne pepper; a weaker alcohol can be used, and this would cheapen the product very much."

From the suddenness of the blindness, the profound condition following and the history of several other men being affected, it would seem as if the presence of foreign materials in the spirit would be the more likely to cause amblyopia.

The fields are most interesting, as I think they clearly indicate the pathologic process that has taken place in the nerve.

FIELDS.

Description of fields:

A. Form field.

B. Blue field.

C. Red field.

D. Relative scotoma for red.

E. The heavily shaded portion, positive scotoma, both for form and color.

F. Lightly shaded portion, relative scotoma (Form is perceived badly, i. e., white appears dirty, not clear, as in other portions of the field).

As no post-mortem examination has been reported in an acute case of this character, and as there is primary atrophy of the optic nerve, I take it that the following series of changes have taken place in the nerve: First, from the profound poisoning there resulted an acute interstitial retro-bulbar nervitis or effusion into the sheath of the nerve, affecting, as alcohol always does, the papulo-macular bundle of fibers and producing, most probably, at first a central scotoma, negative in character (that is, for colors and not for form), with the resulting blurring on the first day. As the neuritis or effusion increased and the pressure on the axis-cylinders became greater, the scotoma gradually changed from negative to positive, and finally became larger and larger until it spread over the entire field, causing complete blindness. After the seven days of blindness, as the neuritis or effusion subsided, the pressure was relieved and vision gradually returned with the exception of that affected through the papulo-macular bundle, which, owing to its depth in the nerve, was more seriously affected by the pressure. The renewed failure of vision after three weeks is to be attributed to a consecutive atrophy following upon the pressure exerted upon the axis-cylinders as a result of the neuritis. This sequence is clearly demonstrated by the interesting fields, showing the "breaking through of the scotoma" to finally meet the periphery of the field, indicating not only an atrophy of the macular bundle, but its extension to adjacent fibers. The portion of the field represented by the letter F shows that the fibers of this region have not undergone complete atrophy, but are damaged so that they will not respond with the same promptness to the vibrations of light as the other portions of the field.

THE PROGRESS AND RESULTS OF PATHOLOGICAL WORK.*

By W. WATSON CHEYNE, M.B., F.R.C.S., F.R.S., London, England,
Professor of Surgery, King's College, London, etc.

Gentlemen: On such an occasion as this one is naturally tempted to look back on the history of pathology during the last sixty years, but I hope that you will not expect me to follow the fashion in this respect. To trace the history of pathology during the last sixty years would be to follow the science practically from its commencement and go back to a state of matters which it is almost impossible for us now to realize. Even going back to the time when I was first introduced to the study of pathology, some twenty-five years ago, the changes which have taken place are enormous. At that time the lectures on pathology consisted practically entirely of morbid anatomy—long descriptions, for the most part very accurate and not materially different from those of the present day, were given of the naked-eye appearances of the diseased parts, but as to how or why these changes were brought about hardly any reference was made. Indeed, very little was known, and when an explanation was attempted it was generally of a mechanical or physical character. At the present time, while, of course, the changes which take place in disease must be known, the study of pathology is especially directed to the discovery of the mode in which these changes occur and the reasons why they appear. To-day it is the etiological side of pathology, so to speak, which occupies our attention, and deservedly so, for, so far as practical results in the treatment of disease are concerned, the knowledge of the changes produced is of comparatively minor importance as compared with that of the reason why they are set up and how they take place. The more accurately we can trace the etiological factors in disease and the subsequent course of events, the more likely are we to arrive at a rational method of treatment.

The most striking and important advance has been the growth of the great science of bacteriology, a science which has not only led to most important practical results, but has also thrown a flood of light on the processes which go on in the body as a whole, and has stimulated research in other directions not immediately associated with it. Twenty-five years ago bacteriology as a science was non-existent. So far as I remember bacteria were not even alluded to in our course of pathology, and it was only from Lord Lister that we heard the real facts so far as they were known, or gained any idea of the importance of their study; but even at that time, although his methods of treatment were already yielding brilliant results and saving many lives, and although he had deduced from clinical facts many points in the life history of bacteria and their relations to the living body, which were only confirmed by experiment subsequently, still the number of actual proved facts was extremely few.

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*An address delivered at the opening of the section of pathology at the annual meeting of the British Medical Association, at Montreal, September 1, 1897.

It is difficult for those who have taken up the subject of bacteriology only comparatively recently to realize the absolute blank which is presented even twenty years ago. When I became house surgeon to Lord Lister in 1876 objections of all kinds were urged against the theory on which Listerism was based, some denying the existence of bacteria at all, others maintaining the theory of spontaneous generation; some asserting that organisms were always present in the healthy tissues, others denying that they had anything to do with disease, or that the success of the antiseptic principle depended in any way on the exclusion of micro-organisms from wounds. It was these objections which led me to take up bacteriology, for it seemed to me of great importance to ascertain whether or not, as the result of antiseptic treatment, organisms were absent from the discharges from the wounds. Although at the present time such an investigation would be one of the simplest, yet when I came to carry it out I was met with the greatest difficulties. Practically nothing of the kind had been done before, and all the means of investigation had to be devised. Methods of staining bacteria had not been introduced, we had no oil-immersion lenses, and I very soon found that by looking at discharges from wounds containing leucocytes, granular matter, and débris with dry or water immersion lenses, and without substage condensers, no satisfactory result could be arrived at. Hence I came to the conclusion that attempts must be made to see whether organisms grew in suitable fluids inoculated from the discharges. Here again everything had to be devised. A suitable pabulum, methods of sterilization, of inoculation, and of incubation had to be worked out. A large amount of time was spent in getting over the preliminary difficulties, and after a satisfactory method had been found much labor had to be devoted to preliminary questions, such as spontaneous generation, morphological characters of bacteria, their presence or absence in the living body, conditions of growth, and so on.

A great advance which followed soon afterward was the remarkable research by Lord Lister on the bacterium lactis, in which he devised a method of separating bacteria by fractional cultivation, which resulted in the separation of the bacterium which causes lactic fermentation from other organisms. By this research, apart from the method, a great step was gained in the proof of the specificity of a particular bacterium, both as regards morphological characters and chemical action.

Then came Koch's work on infective diseases of wounds, and the publication of his methods of staining and examining bacteria and of cultivating them on solid media, and this work is at the foundation of all modern bacteriological research. From this period the investigations have branched off in two directions. In the first place, almost all the infective diseases have been investigated for parasitic organisms, and in a large number the casual agents have been identified. And, in the second place, researches have been carried on in the direction of tracing out the life history and functional activity of bacteria, and of ascertaining what occurs in the body when organisms or their products are introduced.

From the latter point of view we come to another great landmark in the study of bacteriology, namely, Metchnikoff's work on phagocytosis, a theory which is not only very fascinating in itself, but which has proved a most suggestive working hypothesis leading to many of the researches which have given bacteriology its present position. What a remarkable series of views have been opened up in this direction; questions of the relation of the cells of the body to the parasite, the differentiation of cells, alterations in serum, chemiotaxis, the development of the protective agencies and of antibacteric substances, antitoxins, immunity, and so forth. And yet we are clearly only on the threshold; the very simplicity of many of the explanations is, to my mind, sufficient to show that they are incomplete, for the workings of the living body are far too intricate to be summed up in a simple formula.

In spite, however, of all that has been done, many problems still remain unsolved. In the case of tuberculosis, why is it that in one part of the body we have a slow-growing lupus disease, and in another part, perhaps, of the same body a rapidly developing tuberculosis? None of the explanations usually given, such as differences in the structure or resisting power of the tissues affected or of the individual, differences in the virulence or activity of the organism, etc., seems to furnish sufficient explanation. And the same problem is apparently presented with regard to the bacilli of diphtheria, in that we may have in one case a true diphtheria, in another a membranous rhinitis, while in the third, although the bacilli are present in the throat, the individual may be apparently healthy.

Another very remarkable problem is presented by the results which follow free incisions into tuberculous tissue. An incision is made into the abdominal cavity, masses of tuberculous tissue and tubercle are found scattered over the peritoneum; nothing whatever is done, the wound is stitched up, and yet in many cases the patient, who up to that time has been going steadily down hill, begins to pick up, and the disease may come entirely to a standstill. This phenomenon is not limited to peritoneal tuberculosis. When I was a student the tuberculous nature of what are now recognized as tuberculous diseases of bones and joints was then only suspected, and was not generally accepted by the medical profession. At that time Lord Lister, under the impression that these diseases were of a simple inflammatory nature, and having observed the improvement which often followed free incisions into chronically inflamed tissues, began as an ordinary line of practice to make free incisions through the thickened synovial membrane of tuberculous joints with the view of relieving tension, which he looked on as the chief cause of the continuance of the trouble. Nothing else was done, the joint was not even washed out, nothing was taken away, and yet in a considerable number of cases so treated improvement began from the time that the incisions were made, the patients lost their pain, the wounds gradually healed up, and the disease subsided. Even in cases in which the results were not so satisfactory it could, as a rule, be noted that, although the swelling continued in other parts of the joint, and although the disease began again after a few weeks as vigorously as before, yet for a time at least the thickening disappeared in the neighborhood of the scars, showing that there at any rate temporary benefit had occurred. What possible explanation can we give of such a result? The older surgeons used to speak of "setting up a healthy action in the part," and were quite satisfied with that statement. Such a view is too vague for us nowadays, but may there not be here a possible working hypothesis which, if followed out, might throw light on this matter? May not the occurrence of healing processes at one part influence in some way or other morbid processes in the vicinity?

Again, what is the meaning of a chronic abscess? How is it that the tubercle bacilli at one time produce a quantity of tuberculous tissue, at another a cheesy mass, and at another a chronic abscess? I do not for one moment accept the view that we have in the latter case to do with a mixed infection, and that the pyogenic organisms have died out before the abscess was opened.

In connection with the pyogenic organisms also we have many problems. How is it, for example, that after an abscess is opened antiseptically suppuration at once ceases? If instead of opening the abscess antiseptically a poultice be applied, suppuration goes on and may be very profuse. But if it be opened at a time when there is free fluctuation and when it is beginning to point, and if the necessary antiseptic precautions be taken and proper drainage provided no more pus forms, and yet when the abscess is opened it is found to contain living pyogenic organisms. We can easily understand that the subsidence of the fever and general disturbance is due to diminished absorption of toxic products. But why do not these living pyogenic organisms keep up the suppuration? and why is it that after two or three days one may fail to obtain any cultivations from the serum which escapes from the wound? If the inflamed part be opened up antiseptically, however, at an early period, just when pus is beginning to form, there is not always the same complete absence of suppuration, although it seldom goes on to any considerable extent.

Again, an operation wound becomes septic and one naturally opens it up freely and establishes drainage; but here suppuration does not cease at once in the same typical manner as in the case of an abscess which has existed for some days. The old surgeons used to speak of an abscess being "ripe," and they allowed it to remain unopened for some days till it was pointing. They found that if they opened a deep abscess early, suppuration went on and was apt to extend. No doubt a variety of causes led to this dictum: they did not provide proper drainage; they introduced other and more vigorous organisms at the time of operation and afterward, etc.; but do not the facts seem to indicate that, as regards the cessation of suppuration, there is some ground for this idea of ripeness? Is it possible that when the abscess has attained a considerable size and the tension of the pus in it is great the sudden release of the pent-up fluid may lead to such a pouring out of serum containing antitoxic substances as absolutely to destroy the organisms present, while, the case being treated antiseptically, no fresh organisms can come in to take their place? And yet I can hardly think that that is the whole explanation.

Although it is in the department of bacteriology that the most striking advance has been made, great progress has also gone on in pathology generally, in a considerable number of cases no doubt stimulated by the results of bacteriological research, and more especially by Metchnikoff's work. The subject of inflammation has had much attention paid to it as the result of these studies, although I cannot say that to my mind it has been made any clearer. The tendency now appears to be to regard inflammation as the natural effort at repair after injury. At one time this was the view held by surgeons, and the doctrine was that inflammation was essential to healing, and in the description of healing by first intention it was stated that a red blush occurred around the wound, not so severe, however, as where suppuration takes place, and that without this red blush the edges did not adhere. When antiseptic treatment was introduced it became evident that no inflammatory blush or other sign of inflammation was necessary for healing by first intention; in fact, it was found that wounds healed best when no visible sign of inflammation was present. I have, therefore, always taught that inflammation and healing are two different and, indeed, to some extent antagonistic processes, and that although in every wound the tissues in resenting the injury show the early stage of inflammation, yet if no organisms be admitted the inflammatory phenomena soon pass off and where there is destruction of tissue a second process, namely, that of repair, begins. As I have said, in recent writings the tendency appears to be to look on inflammation and healing as parts of the same process; but in spite of the very able arguments adduced in favor of this view I am still unconvinced. I still look on inflammation as the mechanism which gets rid of noxious agents or neutralizes their effects, and on the healing process as that which repairs defects, whether they are caused by injury and associated with inflammation or not. In fact, inflammation must be followed by repair if recovery is to take place, but repair need not be preceded by inflammation. The difficulty arises from the close association of the two processes, both of which have to do with the growth of cells, and from the fact that repair follows as soon as the inflammation begins to subside. Hence under the microscope, except in cases of acute suppurative inflammation, one sees the two processes at work side by side, and it is not a matter of surprise that they should be confounded. As a matter of fact, the more the cellular processes are investigated the more it becomes evident that there is a marked differentiation of cells as regards function.

When Cohnheim first published his observations on the emigration of corpuscles it was thought by many that the leucocyte was everything and did everything, but it was soon evident that other cells of different origin must be taken into consideration. When Metchnikoff's theory of phagocytosis first came out, some of those who adopted it assumed that all wandering cells were phagocytes; but here, again, further investigation has shown that cells differ greatly as regards their phagocytic action. I think that a good deal of the confusion of these processes arises from the fact that expressions involving a teleological argument are becoming very common, more especially with regard to the protective arrangements of the body. On the idea of an acting intelligence on the part of the cells, the two processes would naturally go together; but, looked on as the simple effect of an injury, they should, I believe, be regarded as independent, and the less and the shorter the inflammation the better and quicker the healing process.

A very remarkable thing in connection with these advances, especially in experimental pathology, is the enormous direct practical benefit which has already resulted to the human race; and it is sufficient answer to the antivivisectionists who oppose the use of intelligence and observation and experiment, to point to the saving of human life and the relief of suffering which has taken place in the last few years. Since Pasteur's experiments on spontaneous generation were published, only thirty-six years have elapsed, and during that time remarkable results have been attained in treatment as the result of experimental pathology.

Scientific effort to arrive at the truth as regards the workings of nature are necessarily slow, and must be carried on without any regard to possible ultimate practical results. When the earlier investigators studied the phenomena of electrical action, I doubt if it ever occurred to them that the result of their study would be of any practical value; certainly they could have had no idea of the revolution which the study of electricity would effect in the history of the world. When Pasteur resolved to test the theory of spontaneous generation, it did not seem likely to lead to any beneficial result; indeed, his friends tried to dissuade him from entering into what was apparently a useless investigation, and yet what numbers of human lives have been saved as the result of that work, and what incalculable benefit has accrued! It is greatly the fashion with the opponets of experimental research to demand a single increase in which an experiment has led to the discovery of a means of cure; but in no department of science has a single experiment of itself alone led to the practical result; the final observation which led to the practical result has been built up on numerous and laborious preliminary investigations and observations. And similarly, in regard to the cure or prevention of disease, the final trials on man have been led up to by numerous preceding observations and experiments. If these deluded people had their way the result would be that experiments would be limited to man, and every one to whom a new idea occurred would apply it without any previous investigation—surely an appalling prospect, whether for physician or patient.

The practical results already obtained affect diagnosis, prophylaxis and treatment. The diagnosis of many parasitic diseases has now been rendered certain and easy by searching for the causal organism, and I need only instance such diseases as diphtheria, tuberculosis, malaria, anthrax, gonorrhœa, etc. And it is not only by the discovery of the parasite that diagnosis is assisted, but also by other effects of the organism, such as the sort of changes set up in the tissue, the reaction to products of the organism, for instance tuberculin and mallein, etc., and we shall hear a good deal about one of these diagnostic advances in the course of the meeting, namely, the action of the serum of typhoid patients on motile typhoid bacilli. If no other practical advance had resulted from bacteriological work, the possibility of establishing a definite diagnosis in obscure cases is surely a gain of the utmost importance, for it enables a rational prophylaxis and treatment to be instituted.

The greatest of all the advances, because so wide-reaching, has been in the prophylaxis of disease, especially in the prevention of septic disease after operations, as brought about by the discoveries of Lord Lister. By these discoveries the occurrence of sepsis in wounds made through unbroken skin is prevented, and the chances of general sepsis in septic wounds are much diminished, and numerous lives are saved, not only in this way, but also by the fact that the Listerian treatment permits the performance of many live-saving operations which could not otherwise be attempted. In other cases also, prophylaxis is of great value, as in diphtheria, and in Pasteur's treatment of rabies, which may properly be grouped as a prophylactic rather than a curative treatment.

Lastly, I may refer to advances in the cure of disease. In the case of diphtheria there can be no question that the antitoxin is a most potent curative agent, and that, used in the early stages, it is almost certain to cut short the disease. As regards tetanus, the evidence in the case of animals is absolutely convincing, but in patients suffering from the disease the effect is not certain, probably because we have to do with an acute illness, which runs its course before the serum has had time to act. The same may also be the case with the antistreptococcic serum, although I have great doubts of its value as a curative agent. In other instances, such as plague and snakebite, we may apparently look forward to a cure; while researches are being carried on with regard to pneumonia which may lead to valuable results; nor must I forget to mention Pasteur's system of inoculating cattle against anthrax. What are we to say about the new tuberculin? We all know how careful an observer Koch is, and the fact that he looks on it as a valuable remedy is to my mind sufficient to make it necessary to give it a careful and hopeful trial. I must say that I have always been favorably inclined to the old tuberculin, and have regarded it in relation to phthisis like iodide of potassium in relation to syphilis, and, had I been a physician, I do not think I should have abandoned it in the hurried manner in which it was thrown aside.

But it is not only in the direction of bacteriology that advantage has resulted from pathological research. Look at the advances in treatment from the use of organic fluids. As the result of observations on man and researches on animals as to the effects of excision of the thyroid gland attempts were made to transplant the healthy thyroid gland into animals and patients in whom it was absent, and following these attempts the use of thyroid extract has ultimately come to be a recognized method of treatment. There is, perhaps, nothing more striking in medical treatment than the rapid and remarkable improvement in cases of myxœdema from the use of thyroid extract.

I might enumerate many other instances of direct benefit from pathological research, such as the advances in the treatment of tuberculous diseases of bones and joints as the result of better knowledge of the nature and distribution of the disease, the treatment of appendicitis, and the investigations on peritonitis and diseases of the appendix, the more complete operations for cancer following fuller study of the mode of spread of the disease, and so on; but I have said enough to show the great importance and value of pathological research. Such results must encourage us to go on working with all our might and without ceasing, bearing in mind that many are suffering and dying every day who might be saved if only we had just a little more knowledge of the workings of the body and the processes of disease.

But while pathologists are thus working out problems which affect the general well-being of mankind, and the solution of which can be of no personal gain to themselves, is it too much to ask mankind to furnish the means for such research? The English are looked on as a thoroughly practical people, and yet it is a very remarkable thing that England is almost the only country which does not realize the importance of scientific research, and the result is that in England, with very few exceptions, men who might otherwise have thrown much light on these matters are compelled to turn their attention to practice in order to make a living. Unless work of this kind is done how can we hope to advance with any rapidity in the treatment and cure of disease? The surgeon or physician must wait till the information of which he is urgently in need has been acquired for him by the pathologist. Such apathy can surely be the result only of ignorance. A rich man affected with an obscure or incurable malady cannot understand how it is that he fails to obtain the definite opinion or the relief which he so earnestly desires, and for which he is prepared to pay any price. Surely if he understood the meaning and importance of pathological research, and that the practicing physician can apply and carry out only what is taught by the pathologist, he would bestir himself to aid research in order to gather information which might be of much use to him and to others.—*Medical Record.*

SELECTIONS.

AN ADDRESS ON THE RELATIONSHIP OF PHYSIOLOGY, PHARMACOLOGY, PATHOLOGY, AND PRACTICAL MEDICINE.

Delivered at the International Medical Congress at Moscow on Aug. 7th (19th), 1897,

By T. LAUDER BRUNTON, M.D., D.Sc. EDIN., LL.D. (HON.) ABERD., F.R.C.P., LOND., F.R.S.

GENTLEMEN:—The desire for knowledge which is common to the lower animals and man, savage or civilized, and has induced members of this Congress to come from the ends of the earth in order to gain information must have led primitive man from the earliest times to study the great problems of physiology, the nature of life, of growth, of reproduction, and of death, as well as to notice the connection of the latter with mechanical injuries, such as the wounds inflicted by clubs and spears or by the teeth and claws of wild beasts. Next to the problems of physiology come those of pharmacology, by which I mean the poisonous or remedial action of various substances—mineral, vegetable, or animal. A knowledge of this subject is found even amongst the lowest savages, and is of the greatest use to them, for it enables them, on the one hand, to avoid eating things which may cause discomfort, pain, or death, and, on the other, to obtain food by poisoning waters and thus catching fish, or by poisoning their arrows to kill game which would otherwise escape. Closely associated with the knowledge of the poisonous is that of the curative powers of herbs, and it is possessed by animals as well as man, for cows avoid noxious plants, and dogs will every now and again eat grass apparently as medicine. Primitive peoples use various substances as remedies in disease with more or less success, and one of the most extraordinary points in their practice is that they seem to some extent to have forestalled the newest researches on venins, anti-venins, and organotherapy, for in Africa the bushmen are accustomed to drink the poison of venomous snakes as a prophylactic against their bite, and the Hausas prevent hydrophobia by killing the mad dog and making the man it has bitten eat its liver. The occurrence of death from wounds or poison is intelligible even to a savage, but when illness and death occur independently of these, men naturally attribute them to invisible powers. Thus the Dyaks of Borneo ascribe sickness to wounds from invisible spears wielded by invisible spirits, and during an epidemic of disease in the middle ages the cry often arose that the wells had been poisoned. These crude ideas contain germs of truth, and when we look at Professor Metchnikoff's drawings of Daphnia attacked by a Monospora we seem to recognize the invisible darts of the Dyaks, while during an epidemic of typhoid fever we have often to acknowledge that our wells have been poisoned by bacilli.

It is impossible to trace the steps by which the crude ideas of savage peoples regarding physiology, pharmacology, and pathology, have grown into definite sciences, or even to indicate the most important landmarks, though we naturally think of the names of Alkmaon, Galen and Harvey in physiology; of Nicander, Magendie and Bernard in pharmacology; and of Morgagni, Virchow and Pasteur in pathology. During this century these three sciences have developed with almost incredible rapidity, a complete knowledge of them is enough to tax severely the most retentive memory, and it is almost impossible for any individual to keep up with the advance of all three of them. But just as the whole subject of astronomy became suddenly simplified by a change of standpoint at the very time when cycles and epi-cycles became most bewildering, so at the very time when these three sciences are becoming most complex and diverse they appear to be tending to unification and simplification. Pathology, for example, is now becoming to a great extent a branch of pharmacology, for while a few years ago its chief object was to discover, examine and classify the microbes which give rise to disease, it is now striving rather to discover the nature and actions of the ferments and poisons which they form, and by which they are able to cause disease and death in the animals they attack. Pharmacological investigation, instead of being confined to the alkaloids and other poisons formed by higher plants, has now extended to those formed by microscopic plants or microbes, and thus comes to include a great part of pathology. In the same way, though pharmacology is a branch of physiology, inasmuch as it deals with the phenomena of life as modified by drugs, yet physiology may, to a certain extent, be regarded as a branch of pharmacology, because some of the latest researches regarding the processes of life have been made by pharmacological methods, using the products of animals life instead of vegetable poisons. Amongst the pioneers in this line I may mention my two masters, Kühne and Ludwig, the former of whom by his chemical investigations has enabled us to differentiate the various products of albuminous decomposition, whilst the latter, with his pupils Schmidt, Mühlheim and Wooldridge, discovered the poisonous action of albumoses and peptones, and of the juices of various tissues when injected directly into the blood.

Before the proteid constituents of our food can be absorbed they must be split up during digestion into albumoses and peptones; yet these researches show that the very substances which are necessary to repair waste and are indispensable for the continuance of life prove fatal when introduced into the body in a wrong way or in too great quantity. But the products of the digestion of albumin do not normally enter the circulation as albumoses and peptones. During absorption they undergo changes of a synthetic nature in the walls of the intestine, and probably to a certain extent also in the liver, so that they again form harmless substances, and their poisonous properties are destroyed before they enter the general blood stream. But how is it that the ferments which decompose albuminous food and form poisons from it in the intestines do not pass into the blood and kill the animal by digesting the tissues and forming poisons from them. Of course pepsin cannot do so, as it only acts in an acid medium, but there is no such hindrance to the action of trypsin, and yet it does not destroy the tissues composing the body itself. In all probability the reason why digestive ferments do not digest the tissues is not that they are destroyed in the digestive canal, or yet that they are not absorbed, but that they are altered from active enzymes into inert zymogens which can be stored up without risk, and can again liberate active enzymes when these are required to digest a subsequent meal. In this respect they may be compared to the knives used by wandering peoples to cut up their meat, and which are not thrown away after each meal, but are simply put into sheaths which cover their edges and deprive them for a time of their cutting power.

But it is not in the intestine only that enzymes are found; they are also poured into the blood by the pancreas and probably by the thyroid and other glands. As our acquaintance with the processes of cell life increases it seems more and more likely that the tissue change on which functional activity depends is effected by enzymes, and the truer do the speculations of Van Helmont appear—that life is a process of fermentations. There can be little doubt that if enzymes in a free state were to circulate through the body they would do much harm, and, indeed, we may regard this as well-nigh proved in regard to the enzyme of tetanus. But their action is limited either by their conversion into zymogens or their localization to the cells or tissues where their action is required. This is more readily seen in plants than in animals, and one of the best examples of it is that in germinating wheat. In the ordinary state of the grain the diastatic ferment is kept apart from the starch by a small layer of cellulose, through which the diastase cannot pass, but during germination another ferment appears which has the power of dissolving cellulose, and by breaking down this dividing membrane it allows the diastatic ferment to act upon the starch, and renders it available for the needs of the growing plant.

Enzymes appear to differ amongst themselves nearly as much as albumin, albumoses, and peptones. Some are easily separated from the cells in which they exist whilst others are so closely united to the protoplasm that their separate existence apart from it has been denied. The yeast plant, for example, yields an invert enzyme which can be extracted with comparative ease, but the enzyme which splits up sugar into alcohol and carbonic acid is so firmly attached to the protoplasm of the cell that it is only within the last few months that it has been isolated by Buchner by the application of enormous pressure. It is probable that the enzymes contained in the cells of animal tissues differ in like manner, and that by the use of similar methods we may obtain a number of enzymes with which we are at present unacquainted. But it is not merely the products formed in the digestive canal or in the organs of animals during life, or even the alkaloids that are formed by the higher plants, that act as poisons. The processes of life are much the same in the lowest microbes as in animals or in the higher plants, and these microbes, by forming ferments and poisons, give rise to disturbance of function or death in animals. When grown in suitable media outside the body they produce enzymes and poisons, albumoses and alkaloids, and many of them continue to do so after their introduction into the body.

One of the most curious points, both in the chemistry of the higher plants and of microbes, is that they tend to form at the same time a poison and its antidote. In Calabar bean, for example, we find there are two poisons—physostigmine and calabarine, the former tending to paralyze the spinal cord and the latter to stimulate it, so that each poison to a certain extent antagonizes the other. The same condition is found even more markedly in jaborandi, of which the two alkaloids pilocarpine and jarborine antagonize one another's action so that, although pilocarpine generally greatly predominates, it might be possible to get a specimen of the leaf having no action at all, although it contained a quantity of alkaloids. When injected into animals the toxins formed by microbes and the venins of serpents cause the production of antitoxins and anti-venins, which neutralite their action apparently by chemical combination, in somewhat the same way as an acid and alkali, each poisonous by itself, combine to form a comparatively inert salt. But the two components here, like an organic acid and a mineral base, are unequally affected by destructive agencies, and the anti-venin may be destroyed, so that the venin again regains its activity. The conversion of zymogens into enzymes may be compared to the freeing of venins from their compounds, while the conversion of active venins into inert bodies by combination with anti-venins suggests that a similar process may occur in the case of active enzymes, by which they may be converted into inactive zymogens.

Perhaps the hypothesis I mentioned eight years ago to my pupil and friend, Mr. Hankin, that the germicidal power of organisms is proportional to their power to produce enzymas, may not be altogether unfounded, and possibly we may discover also that immunity, natural or acquired, is nothing more than an extension to the cells of the tissues generally of a power which is constantly exercised during digestion by those of the intestine and liver. This problem is one which pertains to all three sciences and has a most important bearing on practical medicine.

Practical medicine, except when empirical, depends for its advance on physiology, pharmacology, and pathology. A knowledge of the physiology of digestion has led to the satisfactory treatment of dyspepsia by the administration of digestive enzymes, and pharmacological research has enabled us to treat diseases of the circulation with a success previously undreamed of, by teaching us not only how to use aright old remedies, such as digitalis, but also how to apply new ones, such as strophanthus and amyl nitrite, and even to manufacture others, such as nitroerythrol, which possess the special actions we desire, but which are lacking in the drugs we already have. Indeed, new remedies, which shall alter tissue change, lower temperature, relieve pain, and procure sleep, are now being made in such numbers that it is hard to keep count of them. But amongst all the new gains of practical medicine none are so remarkable as those which we owe to pathology. Time would fail me to speak of the prevention and cure of zymotic diseases, but no less astonishing is the discovery that myxœdema depends on inactivity or absence of thyroid gland, and can be cured by the administration of its extract, which seems to act as an enzyme on living tissues, so that the heavy, shapeless features of the patient resume their natural expression, and the sluggish mental processes become quickened. An exhaustive study of enzymes and their products appears to be the most promising way of advancing our knowledge both of the nature and treatment of disease. Probably more is to be hoped for from an investigation into the nature and properties of those enzymes which are intimately associated with the protoplasm of the cells in the various tissues and organs than even of those which are poured into the blood by glands having an internal secretion, such as the thyroid. For all organs, even those which like muscles and nerves are not glandular, have an action on the blood comparable to that of the yeast plant, which modifies the fluid in which it lives by the substances which it removes from or adds to it. It is to a knowledge of the processes which occur in the protoplasm of the cells in the intestinal wall and liver, and of the enzymes by which these processes are in all probability carried out that we must look for an explanation of the conversion of the poisonous albumoses formed during digestion into inocuous albumins, and of dangerous enzymes into harmless zymogens. Moreover, it seems to me that it is by researches into the nature and action of the enzymes not only of microbes, but in the various tissues of the body in higher animals, that we shall learn how the microbes, like the enzymes of the intestinal canal, produce poisonous albumoses, and how the tissues, like the cells of the intestinal walls or liver, convert them into harmless or even protective substances. In this way we may hope to obtain an explanation of toxins and antitoxins, of pathogenesis and immunity, as well as of the nature of diseases unconnected with the presence of microbes, such as diabetes. Twenty-three years ago I attempted to obtain a glycolytic enzyme from muscle, in order to enable diabetic patients to utilize the sugar in their blood. My attempt was unsuccessful, but we may still hope that by other methods we may obtain from animal organs various enzymes, the administration of which may prove as useful in other diseases as the thyroid gland in myxœdema.

Practical medicine depends on physiology, pharmacology, and pathology, but all three are tending to become more and more subdivisions of the wider and all-embracing science of chemistry. It is to a chemist, Pasteur, that we owe the wonderful development of pathology within the last quarter of a century, and we may fairly regard his fellow-countryman, Lavoisier, as the founder of this science. Men from all countries, and especially from Germany, have aided its development; but it seems fitting that at this Congress, in acknowledging our obligations to this science, we should not omit to mention that at its head now stands a Russian, Mendeleef, whose marvelous prescience enabled him to predict the existence of elements which were then unknown and even to describe their properties more correctly than those who first verified his predictions by obtaining the substances themselves. When we consider that little more than a hundred years have elapsed since the time of Lavoisier, and contemplate the vast benefits which medicine and its allied sciences have derived from chemistry during this time, our hopes cannot be otherwise than great for the centuries to come.

CHEYNE-STOKES RESPIRATION PHENOMENA.

Presented in the Section on Practice of Medicine, at the Forty-eighth Annual Meeting of the American Medical Association, at Philadelphia, Pa., June 1-4, 1897.

By N. S. DAVIS, Jr., M.D., Chicago, Ill.

Mr. ——, merchant, 73 years old, of good habits and good family history, came under my care in September last. His health had not been robust for many months. In midsummer his legs became edematous and he then first consulted a physician. He was found to have dilatation of the heart, a slight aortic murmur, atheroma to a moderate extent of the peripheral arteries and interstitial nephritis. His symptoms underwent no material change up to September, when I first saw him. As time went on he grew feebler, thinner and mentally depressed. He had little appetite for food or drink. He was often sleepless. About the middle of October he was too feeble to walk, and rarely slept more than an hour at a time. At night he was tormented by hallucinations which were very persistent. At this time Cheyne-Stokes respiration first manifested itself. The rhythmically increasing and diminishing respiratory movements with short pauses were very noticeable. The periods of apnea were short and the periods of dyspnea were not severe. The pulse was quite regular and was kept at about 85 by strophanthus. The pupils showed no change in size and there was no hebetude during apnea.

Cheyne-Stokes respiration was not constant at this time. It lasted for several days, and disappeared only to reappear from time to time for a few hours or days. Occasionally apnea was almost wanting, and even at the height of dyspnea the respiratory movements were not labored.

In November he improved; respiration became normal, sleeplessness was less troublesome and hallucinations were more infrequent and less persistent. Early in December he grew worse, Cheyne-Stokes respiration reappeared in an aggravated form and persisted for almost a month until he died. Dyspnea was more intense than during the first attack of Cheyne-Stokes respiration and the pauses were longer. His pulse quickened during apnea, and as respiratory movements grew shallower his eyes converged slightly, his lids closed and he seemed to be in deep sleep. If at this time his lids were lifted the pupils were uniformly found to be closely contracted. Conversation with him was slow, for he would cease speaking and apparently become unconscious during the period of apnea. With the first few shallow breaths his lids would open and his eyes would roll slightly as they are apt to do when one is suddenly roused from deep sleep. He would, so soon as respiration was fairly established, resume a conversation without interruption of argument or break in the continuity of events that he might be describing. During apnea the power of speech was lost and mentality seemed suspended. If spoken to when thus apparently dozing he was not conscious of the question. Conversation with him was tedious, for these pauses occurred once in every eighty to ninety seconds and lasted about twenty-five seconds. The dyspnea was very wearisome to him. He was never cyanotic.

I have described this case as it illustrates Cheyne-Stokes respiration in its mildest form, and in that form in which all the accompanying rhythmic phenomena of pulse, eye and mind are present. In 1818, Cheyne (Dublin Hosp. Rep., 1818, vol. ii) first called attention to rhythmically ascending and descending periods of respirations separated from one another by short pauses.

Cheyne-Stokes respiration should be clearly distinguished from irregular breathing accompanied by pauses. In the latter form of respiration pauses occur, followed by several long gasping breaths which may gradually grow less violent and rapid or irregularly become so and cease with the beginning of another pause. Cheyne-Stokes respiration is characterized by a pause of from ten to forty seconds, followed by from ten to twenty respirations, which grow gradually quicker and deeper until they are dyspneic in character, both because of their violence and rapidity. During the succeeding ten to twenty respirations the movements grow progressively less violent and rapid until they cease and apnea begins. Usually the ascending and descending series of movements number about the same, but they are not always equal. The descending series are often less regular than the ascending. In the mildest cases apnea may be wanting, the ascending series of respirations may follow the descending without interruption. If, during the respiratory pause, voluntary efforts are made to breathe the whole chest is lifted by the unusual muscles of respiration, but the diaphragm and intercostals do not move.

Cyanosis is very rarely observable, although the pauses are frequent and many of the respiratory movements are shallow. Patients often complain of the wearisomeness of respiration, but not of hunger for air. In 1859, Reid (Dublin Hosp. Gaz., vi, 308) pointed out the commonly observable changes which occur in the pulse during Cheyne-Stokes breathing.

In most cases during apnea the pulse rate is quicker than during dyspnea; it will beat once or twice oftener in the quarter minute. Not infrequently the pulse is paradoxical. It is apt to be large and soft during dyspnea and small during the respiratory pause. Biot uniformly observed less arterial tension during apnea than dyspnea (Rev. Mensuelle de Méd. et Chir., 1878, ii, 975). While these are the usual changes in the pulse, if any occur, it has been found in rarer instances to be slow during apnea instead of quick, and once Hesky (see Gibson, Edin. Med. Jour., xxxiv) observed almost a complete disappearance of pulse beating during each respiratory pause.

In 1866, Leyden (Arch. f. Path., Anat. und Phys. und f. Klin. Med., xxxvi) first called attention to the rhythmic changes that occur in the pupils during the phases of Cheyne-Stokes respiration. In very many cases, though by no means in all, the pupils become closely contracted during the pause and do not respond to the stimulus of light, although during dyspnea they are normal in size and respond readily to light. Contraction of the pupils usually takes place slowly and progressively, but in a few instances it has been observed to occur in slight successive spasms. Biot thinks these changes are due to variations in blood pressure, which are so often noticeable in the different phases of Cheyne-

Stokes respiration. Recent observations, however, make it probable that they are not to be explained in this way, but are due to the direct influence of the nervous system upon the eye. Often, just as the pupils begin to contract, a slight convergence of the eyeballs occurs and still oftener the lids close during the period of apnea and the patient appears to be in deep sleep.

Not only do rhythmic changes occur in respiration, pulse and pupils but in a proportion of cases, as in the one that I have just described, mental processes seem to be suspended during apnea, although so soon as respiratory movements begin the mind awakes and seems normally clear. In many cases of Cheyne-Stokes respiration patients are deeply comatose and no mental variations are observable. Even in these cases, however, the pulse and eye phenomena are often seen.

In 1876, Ross (Canada Med. and Surg. Journal, v, 544) described a case in which general muscular rigidity occurred during each respiratory pause. Several similar cases have been described since. This is, however, not a frequent concomitant of Cheyne-Stokes respiration, and can not be regarded as one of the phenomena usually accompanying it.

In individual cases we find various combinations of these modifications of pulse, pupil and mental state. They by no means all occur coincidentally. The pulse and respiratory changes are the commonest, and next in frequency the eye and respiratory changes. Gibson (Edin. Med. Jour., xxxiv) has described one case, which is interesting and important, as it throws light upon the nature of these phenomena. It was a case of meningitis, in which at regular intervals there occurred contraction and dilatation of the pupils, and coincident periods of somnolence and waking, but respiration remained normal. This case can not be called one of Cheyne-Stokes respiration, but it illustrates the fact that certain portions of the brain may be involved and produce certain of the rhythmic changes characteristic of Cheyne-Stokes phenomena, although the medulla is not affected. The reverse of this condition, which produces Cheyne-Stokes respiration, is much the commonest.

The phenomena of periodic respiration are very variable in their duration, sometimes lasting only a few minutes or hours, at other times persisting for many days or even for many weeks. An anonymous contributor to the Lancet (1890, i, 776) says that his father, who is advanced in years, has exhibited characteristic Cheyne-Stokes phenomena continuously for many years, although otherwise apparently well.

The dyspnea varies greatly in severity. For instance, in one case which was under my care this winter, the respiratory movements were so moderate that they caused the patient little annoyance, and he scarcely ever complained of difficult breathing. Usually, however, it is sufficiently severe to weary the patients very much, and every now and again it is intense. When respiration is wearisome and difficult it seems to demand relief by treatment. Text-books upon therapeutics and upon medicine give practically no information as to the mode of action or the utility of drugs for the relief of these peculiar symptoms. As one case after another came under treatment, I tried successively the nitrites and soporifics, thinking that they might relieve the dyspnea, as they so often do that of asthma, but I soon found that their effects were uncertain, that apparently they afford relief in one case and none in another. I then tried respiratory stimulants and oxygen inhalations, but with similar results. As during the last year there came under my observation in quick succession a series of these cases, I was prompted to review the literature of the subject with care, hoping that I might there find more information in regard to the nature of the phenomena and its mode of treatment than is to be obtained from our usual books of reference. I have found the literature of the subject very large, but unfortunately, it contains almost no exact information of a therapeutic character. It is chiefly controversial in regard to the nature of Cheyne-Stokes phenomena.

When we approach the treatment of this affection we naturally ask ourselves, under what conditions does it arise and what is its nature? Cheyne-Stokes respiration has been observed as a complication of the most varied maladies, and has been provoked experimentally in very many ways. Edes (Boston Medical and Surgical Journal, 1879, ci, 734) and Cheyne have both pointed to certain families in whom there was apparently an inherited tendency to this peculiar form of respiration. It is rarely associated with such infectious diseases as typhoid fever, smallpox, diphtheria, cholera and whooping-cough. It occurs much more frequently in cerebral affections such as meningitis, apoplexy, cerebral embolism and thrombosis, sun-stroke, insanity, hysteria, hæmorrhage into the cerebellum and medulla, or aneurism in the latter. Complicating hemophilia, hæmorrhage after operations and deep anæmia from other causes has been observed. It is frequently associated with certain circulatory affections, oftenest with fatty degeneration of the heart, valvular disease, pericarditis, aortic aneurism and general arterial atheroma. At times, it also complicates respiratory affections such as bronchitis, pneumonia and tubercular disease of the lungs. It has been observed as a sequel to tracheotomy. In these affections, however, unconsciousness or semi-unconsciousness exists before Cheyne-Stokes respiration develops. It is also rarely observable in severe catarrhal affections of the gastro-intestinal tract. It occurs oftenest of all associated with chronic renal disorders, when it is usually regarded as a manifestation of uræmia. It has been observed in healthy individuals who were sleeping deeply from the effect of prolonged or excessive exertion. It has also been seen when narcosis produced by morphin, chloral and the bromids was deep. In some animals it seems to be the normal mode of respiration during hibernation. It has been produced experimentally in frogs by subjecting them to very considerable changes of temperature, or by holding them under water for an unusually long time. It has been provoked by bleeding and by the complete removal of the heart. In higher animals it has been caused by alternate compression and relaxation of carotid and vertebral arteries; by section of the medulla with or without section of the vagi; by pressure upon the medulla; and by various injuries to the brain and the medulla. It has been produced quite uniformly by section of the latter at the level of the alæ cinereæ. Of all these conditions under which Cheyne-Stokes respiration arises it occurs oftenest in uremia, next in meningitis and other cerebral diseases, next in cardiac, and least in other affections. In all cases there is malnutrition of the brain due either to toxic agents in the blood, to an imperfect blood supply or to other causes.

It must be admitted that the exact nature of Cheyne-Stokes respiration is unknown. Very many theoretical explanations of it have been given. No explanation worthy of the name of theory was propounded until 1869, when Traube (Berliner Klin. Woch., vi., 1869, 277) offered the explanation that in all cases less oxygen was carried to the medulla than was natural, that, therefore, it was less sensitive than it should be. Accordingly more carbonic acid gas was needed to rouse it to activity than under normal conditions. He believed the stimulus of an excess of carbonic acid gas in the tissues was transmitted to the brain by the pneumogastric and other sensory nerves. An unusual accumulation of carbonic acid gas in the system took place during apnea. It stimulated the respiratory center and thus excited dyspnea, but it was soon wearied and apnea reappeared. This theory is no longer tenable, for it has been demonstrated experimentally that Cheyne-Stokes respiration will continue even though the pneumogastrics and all sensory nerves that might transmit impulses to the brain are cut. In 1874, Filehne (Berlin. Klin. Woch., xi, 1874, 152) propounded another theory, which I need not describe for it too has been thoroughly disproven. He believed that rhythmic changes in blood supply to the brain were essential for the production of the phenomena. But it has been shown that Cheyne-Stokes respiration will continue even when the aorta is ligated and a varying blood supply is impossible. In 1877, Hein (Wiener Med. Woch., xxvii, 317) announced his belief that whenever Cheyne-Stokes respiration was produced, vitality generally was lessened and the irritability of the medulla especially so; at the beginning of the pause in respiration, the blood was well oxygenated, but during the pause it gradually became venous; at the beginning of the pause when tissue change was taking place vigorously the respiratory center became more irritable, and as the demand for more oxygen gradually increased it was aroused to activity and produced the succeeding dyspnea, during which the blood was again well oxygenated. This theory, as that of Filehne, is refuted by the experiments which show that variations in blood supply or tissue oxygenation has nothing to do with the phenomena. Moreover, if artificial respiration is maintained during a few of the respiratory pauses, it will not prevent their regular recurrence afterward. In 1879, Luciani (Lo Sperimentale, xxxiii anno, tome xliii, 341) and Rosenbach (Zeit. f. Klin. Med., Band I, 583) independently propounded theories which are somewhat similar, and which to-day seem to be most tenable. They both believe that the respiratory center is automatic, although normally controlled by reflex motives, or by demands due to the condition of nutrition in various tissues or to mental states. Rosenbach believes that when Cheyne-Stokes respiration exists the whole brain is affected, although the medulla is especially so. The normal irritability of the central nervous system is lessened so that it feels influences from without less and its automatic power is more manifest. Its normal periodic exhaustibility is increased; at times even to paralysis. The experimental proof which has been produced, that periodic respiration is not due to irritation of sensory nerves, or variations in blood supply or oxygenation of the nerve centers, is good evidence that the respiratory center is automatic.

That Rosenbach is right in thinking that the whole brain is involved seems probable, because of the complex phenomena which are associated with the characteristic respiratory movements. Ordinarily the medulla is first affected and produces periodic respiration; later other centers are involved and produce the pupillary, pulse and mental changes. The case described by Gibson, and already referred to, in which only the pupillary and mental phenomena of Cheyne-Stokes respiration were present, shows that at times the higher nerve centers may alone be involved or sometimes may be first involved.

Unquestionably, the best treatment is that which is applied to maladies causing Cheyne-Stokes respiration; for instance, the general treatment for uræmia when it is the cause of the respiratory phenomena. Improved celebral nutrition and increased sensitiveness to reflex and external influences might be expected to directly aid the respiratory trouble. Therefore, as good nutrition as possible should be maintained, by carefully regulating diet and correcting digestive disorders in chronic cases of Cheyne-Stokes respiration. Inhalation of oxygen gas has not unfrequently been prescribed, in order to improve cerebral nutrition and make tissue change generally more perfect. Different observers give conflicting evidence as to its utility. In several cases it has seemed to me useless.

In the case which I described at the beginning of this paper it was administered very faithfully during the first period of Cheyne-Stokes respiration, and during the first part of the succeeding three weeks, when respiration was normal. About a week after the inhalation of oxygen was discontinued Cheyne-Stokes respiration reappeared. Oxygen was again used, but not as persistently, for it seemed to have no effect upon the respiratory phenomena or other symptoms. By these methods the attempt has been made to improve nutrition; and simultaneously strychnia has been given to increase the sensitiveness of the respiratory center. In the literature of the subject there is no evidence that strychnia has done good. Indeed, most authors frankly say that it is useless. In uræmia it is contraindicated, for it might hasten the onset of convulsions. There is one instance on record in which it apparently provoked Cheyne-Stokes respiration. This was in the accidental poisoning of a pet dog. The animal was thrown into convulsions by strychnia, but its owner so supported it that the convulsive movements could not be easily re-excited by external influences. Periods of calm were thus produced between the gradually lessening convulsions, in which characteristic Cheyne-Stokes respiration occurred. (Tuke, Edin. Med. Jour., xxxiv, 1888-9.)

Venesection has done good in a few instances. . By it impurities can be removed from the blood, and an overloaded heart and engorged cerebral vessels can be relieved. It has been resorted to chiefly in cardiac and celebral affections.

Various soporifics have been tried. As, however, normal sleep occurring in the course of Cheyne-Stokes respiration does not always modify the respiratory phenomena, so soporifics cannot be expected to uniformly give relief. In some cases, it is true, sleep lessens the dyspnea. In sucn, morphin and chloral are most sure to give relief. Knaggs (Lancet, 1890, i, 744), reports one case that was greatly helped by paraldehyde. Sulphonal and urethan have been tried unsuccessfully. Hyoscin and atropin (Stadelmann, Zeit. f. Klin. Med., 1894, 267) seem to aggravate the phenomena. The nitrites have been used by many observers, who undoubtedly hoped for as prompt relief of this nervous disturbance of respiration as is so usually obtained in asthma when these drugs are administered. Unfortunately they do not produce any more uniform results than the soporifics. In several of my own cases nitroglycerin has afforded decided, although not complete, relief, but in other cases it has been useless. In several I was unable to use it in efficient doses, because of the discomfort which it produced by dilating cerebral arterioles.

Cheyne-Stokes respiration cannot be regarded as always indicative of grave disease, for it occasionally occurs in health when sleep is unusually deep. If it is produced by drug narcosis it is significant of serious poisoning. In renal, in cerebral and cardiac affections, it is always to be regarded as a grave complication, and as usually indicating approaching dissolution. It is least significant of gravity when it is most chronic. It is very frequently completely recovered from. This is oftenest true when it occurs in connection with infectious and cardiac diseases.—*The Journal of the American Medical Association.*

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ABSTRACTS.

Eventration in Children.—Budinger (Wiener Klinische Wochenschrift, 1897, No. 21; Gazette Hebdomadaire de Medicine et de Chirurgie, August 8, 1897) reports a number of cases in which boys from five to nine years old, previously in perfect health, have become the subjects of persistent dyspeptic troubles (N. Y. Med. Jour.). The appetite is impaired and they decline certain articles of food which are particularly apt to engender gas and consequent epigastric pain, such as bread and potatoes. From time to time, in the midst of their play, these children are seized with gastric colic. They suddenly stop playing, turn pale, and bend forward, at the same time pressing with their hands on the pit of the stomach. In every such case the author has found separation of the recti muscles extending from the ensiform cartilage to the navel, their inner edges forming the boundaries of a lozenge-shaped space. All the symptoms have been controlled by the application of overlapping strips of diachylon plaster as wide as one's finger. If this irritates the skin, salicylated-soap plaster may be used instead.

HYDROZONE FOR DISORDERS OF THE GENITO-URINARY TRACT.

Dr. John Aulde, of Philadelphia, (Medical Times and Register of Philadelphia, Pa., Dec. 5, 1896), states that about eight years ago he was forcibly impressed with the value of peroxide of hydrogen in a protracted case of gonorrhœa. The disease had persisted for three months, despite the treatment of several attendants, there being a constant discharge, and, in addition, there was an orchitis present, the left testicle being about as large as a baseball. Treatment consisted of the local use of injections of equal parts of Peroxide of Hydrogen and moderately warm water, used at intervals of four hours, these injections being followed by a solution of arsenite of copper containing one milligram (one 65th grain) to the drachm, diluted with an equal quantity of hot water.

In a week the patient was able to return to his home in a distant State, the discharge from the urethra having entirely ceased, and pain and chordee having disappeared.

The author advises the same treatment for non-specific urethritis, and gleet, but as hydrozone is much stronger (2 times) than the peroxide, and perfectly harmless, he gives it the preference.

In vaginitis and vaginismus this treatment is of especial value. The treatment heretofore recommended by physicians, consisting of hot vaginal douches, either with or without some alkali, as sodium bicarbonate, followed by the injection of a small quantity of peroxide of hydrogen (medicinal), in warm or cold water, is superseded by the single application of a hot solution of hydrozone, one part in eight; the patient should use a fountain syringe which should be hung upon the wall about six feet from the floor; the patient sits upon a suitable vessel, and introduces the rubber tip of the hose well back into the vagina, while the labia are compressed by the disengaged hand; this allows the solution to so distend the vagina as to bring it in contact with all the diseased tissue. The injection should be repeated twice in twenty-four hours.

In uterine diseases, where the solution must be brought into contact with the endometrium, the following treatment is pursued:

The patient is placed in the dorsal position, with the hips well elevated; an ordinary dilator is employed to distend the cervix, so as to admit the nozzle of the syringe and permit the free egress of the injected fluid; (a suitable return flow tube can be used to better advantage, the Fritsche's douche is the best that can be used). The injection is then made, a liberal amount of the hot medicated solution being used.

There is need of caution in chronic cases, that the effervescence which attends the destruction of the unhealthy tissue does not force some of the debris into the Fallopian tubes. This is best avoided by using a large quantity of the solution, and afterwards directing the patient to assume the upright position. The pressure thus brought to bear upon the uterus will cause the complete discharge of all debris.

A preliminary vaginal douche should always be taken, using the medicated solution, as, otherwise, harm might ensue by the entrance into the uterus of the vaginal secretions. The author warns against the use of the vaginal douche if the cervix be patulous, as there is an almost certain danger of the infected vaginal debris being forced into the uterine cavity. To avoid this the vagina should be cleansed by the local use of the medicated solution through the speculum.

The author believes Hydrozone to be the best remedy for cystitis occurring either in the male or female. The bladder should be washed out with the solution (one to eight) a small quantity being used at first in chronic cases, owing to the painful muscular contractions following the withdrawal of the solution. The amount can be gradually increased. (A double current hard rubber catheter should always be used for that purpose.) In gonnorrhœa, gleet, and cystitis, the local treatment is oftentimes aided by the internal administration of hourly doses of calcium sulphide, one-tenth of a grain.

WHEN IS THE RIGHT MOMENT FOR BEGINNING THE TREATMENT OF SYPHILIS.

Prof. Schwimmer (Budapest).—I am in favor of the earliest possible treatment, so as rapidly to combat the chloro-anæmia and the various disorders in the elimination of hæmoglobin, both of them accidents of the first stages of the affection. A treatment of two years' duration is generally sufficient.

Dr. Watrachewsky (Warsaw).—I am distinctly in favor of expectation, in consideration of the evolutionary nature of syphilis and of its very different varieties, latent or apparent. Mercury has no action on the latent form of the disease, simply barring the way to the elimination of the morbid agent. Malignant syphilis, in 70.4 per cent. of the cases, is met with in patients treated early, and this afterwards leaves the physician unarmed, the patient being no longer able to bear mercury.

The remedy can only be dosed with method and precision when we are in presence of well-marked manifestations of the disease. We need hardly ever mention in this connection the doubtful cases in which neither the physician nor the patient are sure whether or not they are in presence of syphilis.

The action of mercury is rapid when skilfully handled: the treatment need generally not last more than from four to eighteen months; there is no need for the *ante-nuptias* cure, which affords no security to anybody except the physician.

Repeated courses of treatment in the absence of all lesions, especially so when they are continued for a long time, are a great source of danger for the organism; they prepare or actually set up medullary or cerebral lesions.

Dr. Jullien (Paris).—During the last thirty years, gradually all authors have come to accept the necessity of the earliest possible and, I may add, the most energetic possible treatment of syphilis. I upheld this same opinion before the Rome Congress, when I indicated the basis of this intensive and early treatment.

The first observations on the abortive effect of the intensive and early mercurial treatment on the evolution of syphilis were published by Diday, then by myself. Later on, in 1894, Deutsch ascertained that the reflexes remained intact during the rubeolic period, and then again he recognized the absence of neurasthenia in syphilitic patients who had been subjected to the mercurial treatment *ab initio.* All these results agree and demonstrate the good effects of early treatment.

The more modern methods of treatment, I refer particularly to calomel injections after Scarenzio's method, afford results which could hardly have been foreseen with the old pill-method of treatment: I mean the prevention of the roseolar exanthemata, which used to be considered as inevitable. They become purely exceptional with Scarenzio's method, and visitors to my wards at Saint-Lazare are always struck with the rarity of these eruptions. The cycle of the malady is disturbed, the exanthemata change from their wonted appearance, the various periods are suppressed or shortened. This result is never missed when the treatment is carefully followed for a year, or even longer if necessary. It is probable also that the virus, having been constantly fought and weakened from the very beginning, will, in the later stages, lack the necessary strength to create a new morbid phasis.

To conclude, I should say that every syphilitic patient ought, from the very first appearance of the chancre, to be subjected to the severest specific treatment, and also that the practice of the calomel injections ought to be currently resorted to, so as to meet this most urgent of all the indications of the case. I am daily becoming more and more deeply convinced that the true treatment of syphilis ought to be instituted before the disease breaks out.

Dr. Barthélemy (Paris).—Mercury must be given as soon as the diagnosis of syphilis becomes well established. Whenever, on the other hand, there is any doubt at all as to the true nature of the case, all specific medication must be abstained from. I consider these two fundamental rules to be absolutely binding on every practitioner.

The early exhibition of the specific medication is beneficial for the patient; the accidents to which he is exposed become lighter, more superficial, less frequent, less liable to recurrence, more transient. It is eminently advantageous for the patient's family as well, the chances of contamination being thereby reduced to a minimum. To act quickly in such a case is perhaps to protect from contagion the present and the future family of the patient.

Dr. Caspary (Königsberg).—The treatment of syphilis ought, in my opinion, to be begun without delay, the moment the first signs of the disease show themselves on the face and the mucous membranes. The chancre, in certain rare cases, presents very intense inflammatory lesions against which the local treatment remains perfectly helpless; in such cases the general treatment must be applied without any hesitation, even in the absence of all secondary phenomena. The specific medication must, in the beginning, be continued long after the disappearance of all the symptoms of the affection.

The hypodermic method ought always to be chosen in preference to the internal, and it will be prudent not to renew the treatment, except on the reappearance of the symptoms.—*Medical Week.*

ANTITOXINE IN TUBERCULOSIS.

Resume of Reports on Paquin's Antitubercle Serum.

The use of antitoxine in tuberculosis, variously called antitubercle serum and antituberculin, prepared by Paquin of St. Louis, has been receiving vigorous trial throughout the Union during the last two years, and in suitable cases substantial results have been reported. A summary of clinical reports follow:

Paul Paquin, M.D., St. Louis, Mo. (N. Y. Medical Journal, 226 cases. October 24, 1896.)

40 recoveries that seem permanent.
3 recoveries apparent lesions in statu quo.
110 improved.
41 disappeared from observation.
32 deaths.

Frank T. Chamberlin, M.D., Washington, D. C., Physician in Charge, Throat and Chest Service, Eastern Dispensary, etc. (Va. Med. Monthly, March, 1896), read before Medical and Surgical Society, District of Columbia, 15 cases. All improved. Three different manufactures of serum used. Dr. Chamberlin states that in suitable cases "with a strong serum one can undoubtedly combat tuberculosis."

Dr. J. A. Dunwoody, Cripple Creek, Colorado (New York Med. Record, July 11, 1896), four cases treated with antitubercle serum. Endorses serum strongly; includes his own recovery.

Dr. L. L. Shropshire, San Antonio, Texas (N. Y. Med. Journal, January, 1896), reports recovery of case with hæmorrhage, purulent expectoration and also recovery of a case of pure tuberculosis, and numerous improvements.

J. Lemen, M.D., Professor Physical Diagnosis and Therapeutics, Marion Sims College of Medicine, St. Louis (N. Y. Med. Journal, September 21, 1895), reports recovery of acute case of tuberculosis from desperate illness.

Geo. W. Cale, M.D., Surgeon to Womans' Hospital, St. Louis, before Mo. Medical Society (St. Louis Medical Review), reports recovery in case of bone tuberculosis.

A. M. Hayden, M.D., Surgeon St. Mary's Hospital, Evansville, Indiana, reports ten cases with several recoveries, including a case of tuberculosis of the liver and one with serious cavity. (Before American Medical Association, at Atlanta, 1896.)

Wm. Miller, M.D., Boerne, Texas, reports case of pulmonary tuberculosis, second degree, complete recovery.

Hoell Tyler, M.D., Mentone, Cal. (Southern California Practitioner) reports recovery of himself and others from tuberculosis.

Dr. C. H. Mitchell, Carbondale, Ill., reported two recoveries to Southern Illinois Medical Society (North American Practitioner, August 2, 1896).

John M. Allen, A.M., M.D., Prof. Principles and Practice of Medicine, University Medical College, Kansas City, Mo. (Journal American Medical Association, May 29, 1897), reports four cases, with good results in three cases.

Wm. Hudson Prioleau, A.M., M.D., Summerville, S. C. (N. Y. Med. Journal, June 26, 1897), 13 cases, 2 cures, 10 benefited, 1 refractory.

Dr. Landon B. Edwards, ex-President Richmond Academy of Medicine and Surgery (Va. Medical Semi-Monthly, August 11, 1896) reported 14 cases; 2 discharged apparently cured, 7 improved, 3 in statu quo, 2 hopeless.

Report to American Medical Association, Annual Meeting, Philadelphia, Pa., Paul Paquin, M.D., St. Louis, 67 cases not previously reported; recoveries, 17; considerably improved, 35; improved to lesser degree, 11; fatal issue, 4. (Journal American Medical Association, July 17, 1897.)

Dr. J. McFarland, Prof. Pathology, University Pennsylvania (N. Y. Medical Record, June 10, 1897), using antituberculin from the ass, 20 cases; 1 cure, 10 very encouraging and improved, 7 unchanged, 2 worse.

Other physicians reporting recoveries or provisional cures are:

William E. Ground, M.D., West Superior, Wis.; Richard Fricke, M.D., Fort Wayne, Ind.; C. E. Bauer, M.D., St. Louis, Mo.; J. D. Dillin, P.O., Cal.; A. E. Powell, M.D., Marion, Ind.; W. A. Garner, Esq., Editor Texas Stockman, Fort Worth, Texas; W. P. Sperry, M.D., Phillips, Wis.; J. H. Soper, M.D., San Francisco, Cal.; P. L. Markley, M.D., Rockford, Ill.; J. L. Wiggins, M.D., East St. Louis, Ill.; Ozias Paquin, M.D., St. Louis, Mo.; J. H. Rippey, M.D., Pierson, Iowa; A. G. Deardorff, M.D., San Francisco, Cal.; E. A. Wood, M.D., Sedalia, Mo.; J. J. Norwine, M.D., Bismarck, Mo.; S. Lane Anderson, M.D., Chadd's Ford, Pa.; W. R. Beatie, M.D., Everton, Mo., etc., etc., etc.

An important review of Sero-Therapy, by Austin O'Malley, M.D., Ph.D., LL.D., Washington, D. C., appears in "Reference Book of Practical Therapeutics" of 1897, edited by Frank P. Foster, M.D., Editor New York Medical Journal, Vol. II., pp. 179 et seq., which will interest those studying sero-therapy.

THERAPEUTICS.

AN IDEAL FOOD IN GASTRO-ENTERITIS.

In gastro-intestinal affections attended with profuse diarrhœa the problem of feeding the patient often assumes more than ordinary importance in view of the extreme exhaustion due to the constant drain of the albuminous materials from the body. The remarks on this subject by an editorial writer in the Lancet, March, 1897, are well worthy of attention, as they suggest the means of solving this problem in a large number of cases of gastro-enteritis. The author says: "When the patient's stomach will not tolerate food in solid or liquid form; when the water that is craved is immediately vomited, whatever the disease that may be the cause of the condition, the physician's problem is a difficult one. When we consider the large number of death certificates in which asthenia appears as a secondary cause (remembering that this is merely another term for general exhaustion) it must be admitted that the lives of many people might be saved if they have proper nourishment. Milk is probably nearest to the ideal food for the sick, but many people cannot drink it even during health, and with them it is not to be thought of during illness. In other cases the quantity necessary to sustain life is an objection to its use. Of the various prepared foods Somatose is certainly the most acceptable. It is prepared from meat, is rich in albumin, ordorless, practically tasteless, and freely soluble. In gastro-intestinal diseases it has proven particularly beneficial, arresting nausea and vomiting, and promoting appetite for other foods. It may be used for rectal feeding after the addition of a little common salt to a solution in water or milk.

TUBERCULOSIS PERMANENTLY CURED BY SERUM.

By J. A. DUNWOODY, M.D., Cripple Creek, Colo.

As to the permanency of the relief of symptoms of tuberculosis in those cases in which I have used the anti-tubercle serum (Paquin), I have to say, referring to cases on which I have made a previous report:

In case No. 1, the last injection of serum was administered on March 20, 1896, since which time all symptoms have remained relieved, that is, there is no cough whatever; temperature has remained absolutely normal and weight has increased to one hundred and fifty-eight pounds, and remained at that point. There are no points of solidification, and the vesicular respiration is normal, which fact was revealed upon through physical examination. The patient expresses himself as not having been in as good health, even before he was attacked with tuberculosis. There can be no doubt about this case having been one of tuberculosis, as the diagnosis was confirmed by Dr. William Pepper of Philadelphia, and subsequently by the late Dr. Alfred L. Loomis, of New York. In addition to this, there have been several tests made of the sputum, and in every instance the microscope revealed the presence of the tubercle bacilli.

Case 2, H. H. Has not had an injection since March 1, 1896. All symptoms of tuberculosis have disappeared; no cough whatever; the temperature has remained normal, and the weight has increased to forty-two pounds. The little patient is quite robust and in most excellent health. Upon physical examination I find that the left lung, where the lesion had been, is quite clear, no abnormal sounds, the vesicular respiration free, easy and perfectly normal.

Cases 3 and 4, respectively, have not been relieved, as each of the other cases; have discontinued the use of the serum of their own volition, though it is doubtful if they would have ultimately been cured for the reason that each one had the mixed infection. In case 4, as I have stated in a previous report, he was addicted to the disgusting habit of masturbation.

I feel satisfied that should the anti-tubercle serum be employed in those cases of tuberculosis before they have the mixed infection, that the results would be far more satisfactory than by any other known line of treatment.

THE WOODBRIDGE TREATMENT OF TYPHOID FEVER.

Dr. Woodbridge recommends that on the appearance of the earliest symptoms of typhoid fever, or any pathological condition in which a general or intestinal antiseptic or eliminant is indicated, the treatment to be begun with the following:

TABLET NO. 1.

Podophyllin	1-960 gr.
Hydr. chlor. mit.	1-16 gr.
Guaiacol carb.	1-16 gr.
Menthol	1-16 gr.
Eucalyptol	1 min.

One tablet of the above formula should be given every fifteen minutes during the first twenty-four hours, and in larger doses the second twenty-four hours, if found necessary, so that during this and the succeeding twenty-four hours there may be secured five or six full and free evacuations of the bowels during each of these periods. On the third or fourth day the following treatment should be begun:

TABLET NO. 2.

Podophyllin	1-960 gr.
Hydrarg. chlor. mit	1-16 gr.
Guaiacol carb	¼ gr.
Menthol	1-16 gr.
Thymol	1-16 gr.
Eucalyptol	1 min.

One tablet to be given every hour or two.

Prescription No. 1, as well as prescription No. 2, should be given very freely at first, gradually reducing the frequency of the dose, the object being to gradually reduce the number of movements of the bowels until the temperature has dropped to normal, when the movements should be only one or two a day.

Should symptoms of ptyalism manifest themselves, the treatment should be promptly discontinued for a day or two, sodium or potassium chlorate prescribed, returning as soon as possible to prescription No. 1 or prescription No. 2. About the fourth or fifth day the treatment with prescription No. 3 should be begun:

NO. 3 CAPSULE.

Guaiacol carb	3 grs.
Thymol	1 gr.
Menthol	½ gr.
Eucalyptol	5 mins.

One capsule every three or four hours, alternating with prescription No. 1 or prescription No. 2.

During all the course of treatment the patient should wash down the medicine with large quantities of distilled or sterilized water, or, in case it is indicated, some good laxative or diuretic mineral water is applicable to the case. It is claimed that if this treatment is begun early nothing more will be required, and, if carefully and intelligently carried out, will rarely fail to abort typhoid fever.—*Medical Summary.*

XEROFORM IN MINOR SURGERY.*

By Dr. THEODOR BEYER, of Vienna.

My experience, gathered during a year's active practice as staff-physician, though limited and without claim to completeness, may interest the general practitioner; with the active aid of Staff Surgeon S. Stoekel I was enabled to test the well-recommended xeroform in a considerable number of surgical cases occurring among the men of two artillery regiments. The majority of the cases required the use of an antiseptic in powder form, there being few aseptic operation wounds, but generally infected excoriations, suppurative processes of the skin and glands, and unclean wounds such as are due to handling of the heavy artillery in the field, on extended marches and manœuvres, handling of horses, etc.; on the other hand the conditions for treatment were unfavorable (in farm houses, on the road, in fields, etc.) I may, therefore, without meriting the charge of unjustified optimism, ascribe the favorable results to the xeroform treatment.

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* Translated for the American Therapist from the Wiener Medizin. Blätter Dec. 24, 1896.

Xeroform (Tribromphenol-bismuth) is a nearly odorless, yellowish, non-toxic, fine powder, having reputed bactericidal properties. It is not affected by air or light, and can be sterilized. Hueppe employed it successfully as intestinal antiseptic during the cholera epidemic at Hamburg. He ascribes its efficiency to the splitting up into its two components, the bactericidal tribromphenol and the styptic bismuth oxide. The same action may be supposed from its contact with the alkaline traumatic secretions.

Of my cases I will first mention shoe-pinching and the resulting ulcerative processes. After careful cleansing with soap and 3 per cent. boric acid solution, removing the hardened secretions and trimming of the edges, I covered the wound with a thin layer of xeroform and this with a patch of official adhesive isinglass tissue, or in some cases with 5 per cent. xeroform plaster-mull. Successful healing was quickly achieved, two or three renewals of the application usually sufficing. It seemed to me that the notable reduction of secretion was the best evidence of the favorable action of xeroform. Copious suppuration under the bandage was very rare, but cicatrization proceeded rapidly and without local pain. I must say, that it is necessary to clean the floor of the ulcer thoroughly, for which purpose I employed strong carbolic solutions and a sharp curette. The same favorable results were observed in extended cases; by fully covering the excoriations—doubly painful during summer heat—with a 30 per cent. xeroform gauze, healing followed surprisingly fast.

In the treatment of suppurative processes of the skin (furuncles, abscesses, felons) I also proved the notable astringent-siccative effect of this agent. The best proof of this was demonstrated in one case (abscess formation of a blood tumor caused by a horse's kick) requiring an incision of nearly 4 inches in length, which I covered—after washing with a boric acid solution—with sterilized gauze; the secretion on application of xeroform was reduced to a minimum, granulation was strong and healthy and the surrounding epithelium was unaffected. A Thiersch transplantation completed the case in satisfactory manner. As regards the effect of the drug on the healthy skin, I never observed an undoubted xeroform-eczema. For control I bandaged skin surfaces in the same manner as corresponding wounds, but the skin always remained unaffected.

Of fresh wounds I may mention saber cuts, bullet wounds, burns from boiling water, and a large number of lacerated contusions. The former are mentioned only to complete the record; they were healed by simply bandaging with xeroform gauze. The latter, as infected wounds (caused by being kicked or falling from horses, and aggravated by the rubbing in of cinder dust, sand, manure, etc.), appear more convincing as proofs of the value of xeroform. Treatment consisted of correct cleansing of the wounds with soap and boric acid solution, a stitch when necessary, and application of xeroform with blower and by sterile bandage (sterile white gauze used exclusively). The results were highly satisfactory, with never a case of suppuration.

I had little chance for treating venereal affections. Two cases of ulcus molle healed promptly without complication; balanitis and balanoposthitis disappeared as quickly. Of cutaneous affections I treated several cases of eczema of the auricle and face; after repression of the acute inflammation, xeroform applications achieved complete healing in a few days. Even more favorable was the effect in intertrigo; within two or three days every trace of the affection disappeared, and this treatment proved vastly superior to any of the usual salves or dusting powders.

The reports of its favorable effect in acute intestinal catarrh are too well known to require confirmation on my part. (I treated several cases of cholera morbus with three to five doses of 0.5 xeroform with most excellent results.)

A brief summary of my observations is, therefore, entirely favorable. Xeroform is undoubtedly an effective antiseptic; it has the advantage over iodoform of being odorless and sterilizable; it aborts secretion, promotes cicatrization, does not irritate the epithelium; it may even prove valuable in eczema treatment. Its application in minor surgery accordingly appears very appropriate.

THE THERAPEUTIC ACTION OF ORPHOL (BETANAPHTOL-BISMUTH).*

By Dr. GOLINER, of Erfurt.

Intestinal ulcerations, occasioned by the breaking down of cheesily degenerated follicles, are of not infrequent occurrence during the progress of pulmonary tuberculosis, and, as is well known, are found most often in the ileum. Thence the disease spreads to the colon and even to the vermiform appendix. The number of the follicles affected varies in different cases; sometimes the gut is fairly sown with them over a large extent of its surface, and sometimes the malady is limited to a small region. When the cheesy central mass softens and is extruded, a follicular ulcer is formed, whose tubercular nature is shown by the presence of granulating connective tissue containing miliary tubercles in its neighborhood. The central necrosis progresses and the ulcer extends, whilst connective tissue, new growth and tubercle formation advances at the margins, and thus the follicular lesions soon develop into ulcers. Especially noticeable among the symptoms that they cause is the obstinate and violent diarrhœa, more especially when the ulcers are situated in the rectum or lower colon. Irritation of the intestine of any kind almost always causes increased frequency of defecation.

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* Translated from The Allg. Med. Central-Zeitung, 1896, No. 96.

The thinness of these stools is accounted for, according to Nothnagel (Zur Klinik der Darmkrankheiten, 1881), partly by the pathological secretion from the ulcerated surfaces, and partly by the lessened power of resorption of the diseased intestinal wall, in consequence of which the fæces are less solid than usual. When in the course of a consumption there appears an obstinate diarrhœa, perhaps mixed with blood and accompanied by colicky pains, it is extremely probable that intestinal ulceration is present. If peritonitis symptoms appear, then the ulcerations are approaching the serous membranes, or they have already perforated the wall of the gut.

Our attempts to influence the diarrhœa of consumptives by intestinal antisepsis have lately been given a rational basis by Chaumier, of Tours, who has called renewed attention to the importance of these therapeutic measures. Daily experience teaches us that astringents, like opium and tannin, have no healing influence on tubercular intestinal ulcerations. What is required is a reliable intestinal antiseptic that shall have at the same time astringent properties, thus lessening the evil effects of the micro-organisms and their products, and making the mucous membrane a bad culture ground for them. The lessening of the secretion also reduces to a minimum the nutrient materials obtainable by the microbes. Orphol or Betanaphtol-Bismuth is such a drug. It is a light-brown powder, prepared by the chemical factory of Von Heyden, at Radebeul, and, though composed of oxide of bismuth and naphtol, has neither the repulsive smell nor the burning taste of the latter drug. It has an agreeable, faintly aromatic taste and smell, and contains 20 per cent. of betanaphtol and 80 per cent. of bismuth oxide. It thus combines an active antiseptic with a powerful astringent agent. And since in most cases of chronic intestinal catarrh abnormal fermentation and decomposition plays a great part, the disinfection of the alimentary canal is an important therapeutic indication. The flatus and distension of the abdomen, the colicky pains, the abnormal abdominal sensations, all these symptoms are due to the activity of the micro-organisms in the intestine. I had occasion some time ago to try orphol in a series of cases of lung disease that suffered from chronic intestinal catarrh. The result was an entirely good one. The stools became formed and regular, and the troublesome symptoms above mentioned disappeared. I select the following case from the series:

A phythisical patient, 52 years old, had complained for some time of flatus, abdominal pains, and persistent diarrhœa. The number of thin stools varied from four to ten daily, with occasional attacks of colic. Although the patient had remained in bed for weeks, and had taken opiates, the troublesome diarrhœa persisted. He was emaciated and anæmic, and the meteorism was marked, though there was no especial sensitiveness to pressure. The stools were almost watery, dirty-brown in color, foul of odor, and were plentifully mixed with mucus, with occasional streaks of blood. I was evidently dealing with chronic enteritis and ulceration of the intestinal mucosa. I put him on a bland diet, and gave him 1.0 gm. (15 grains) of orphol after eating, and as this was repeated every two hours, he took 5 gm. (75 grains) daily. The result was a favorable one, the borborigmi eructations and pain were relieved, and three days later constipation set in. The appetite, formerly very bad, improved, and the patient began to gain in weight. Later his bowels moved once a day and the stools were of the consistency of thick gruel.

Orphol influences the mucous membrane of the digestive tract in two ways. In the first place it is a disinfectant, hindering the development of the bacteria. It is also an astringent to the mucosa, in virtue of the bismuth that it contains. It is worthy of notice, also, that orphol does not, like opium and tannin, in any way interfere with the stomach, so that even patients suffering with dyspepsia bear it very well. To Chaumier is due our first knowledge of its antiseptic and astringent properties (com. E. Chaumier, De l'emploi de l'Orphol dans l'antisepsie intestinale, Tours, 1896), and he explained the theory of its pharmacodynamic action. He showed that Betanaphtol-Bismuth was decomposed in the intestinal canal into naphtol and bismuth, the first being antiseptic and the last astringent. A small portion of the naphtol is excreted in the urine; the rest passes out with the fæces.

The new intestinal antiseptic is worthy of the attention of the practitioner.

RECTAL GONORRHOEA IN WOMEN.

In the past year Dr. Baer has received in his service 296 women with affections of the genital organs. Of these, 105 had syphilitic lesions exclusively; 191 presented simple blennorrhœa. Of the 191 with gonorrhœa, 67 presented the affection in the rectum.

Theoretically, according to the author, the infection occurs in three different ways: Directly through abnormal coitus; indirectly from the communication with the rectum of an organ attacked with gonorrhœa; by the conveyance of the germs from without by pus, or by therapeutic measures (thermometry, irrigation, etc.).

As to the first, the objective symptoms (form of the anus) are very inconstant, and one cannot always believe the patient; the second mode is rare; the great majority are caused by carrying toward the rectum the infectious fluid from the vulva. The urethral discharge frequently is not of sufficient copiousness to make it responsible; but this is not true of the discharge proceeding from the cervix in metritis. Infection occurs with greater facility in prostitutes and in women who have been pregnant numerous times.

Some patients complain of marked pain on defecation; others discharge both pus and blood via ano. The mucosa is congested, and bleeds easily in consequence of erosions produced by maceration and epithelial desquamation. Speculum examination reveals lustrous folds of the mucosa, is but slightly painful, and there are no disorders of defecation.

Treatment consists of injections of three-per-cent. solution of boric acid, and to diminish the secretion and destroy the virus, solution of nitrate of silver 1 to 3,000, employing a quart of each solution. This should be continued for from five to six weeks. When an exacerbation occurs, it is difficult to decide whether or not a new infection has taken place.—*Medical Standard.*

MISCELLANEOUS.

MEDICAL SERVICE ON THE OCEAN LINERS.

Every season many physicians in the interior write to New York and other seaport cities for a temporary position as surgeon on some ocean liner, thinking that their local reputation and experience will command an opening at once. The impression is given that most of the surgeons are young men who only serve a short time until some better place is opened. While this is true in part, the fact is the medical service on a liner is really very difficult, and requires a peculiar talent not common in the profession, especially among young men. Even old experienced physicians cannot readily adapt themselves to its requirements. The physician in such a position must be, if successful, a highly educated man of the world, familiar with society, a good conversationalist, with tact and excellent knowledge of human nature; even able to entertain his patrons as well as to give them medicine, with exact knowledge of their whims and follies, and with ability to cater to them without losing his personal influence over them. Next to the captain the surgeon must be the most popular man to please the passengers. He must possess a rare fund of information, and ability to impart it at the right time and place. He must have rare patience and skill to humor and retain the confidence of the seasick when all the worst elements of their nature are uppermost. The despair and intense depression, with reckless indifference of surroundings and consequences which characterize the seasick, require psychological skill not common to even specialists. The surgeon becomes the confidant of every one at such times, and must have tact to retain this by personality as well as by medicine.

Many of these liners depend for passengers on the reputation of the surgeon. The confidence he has created has built up a patronage which will come to this ship as long as he is surgeon. Invalid men and whimsical women will refuse to go on any other ship, and practically many of these steamers are popular more on account of the surgeon than from any other conditions. Ship managers recognize this in part, yet they rarely pay the real value of such services. From one to two hundred dollars a month is the stated salary, and the expectation is that an equal amount will be picked up from the passengers.

From the moment of leaving port the surgeon's entire time is taken up with the passengers, socially and medically. The latter is a study for temporary results to keep up the person until land is reached, to put off and temporize with conditions of mind and body for the present. Every thing is limited to a week; a broken bone must be set at once, and a hemmorrhage checked, but every other condition can be put off until the ship lands. To do this requires infinite tact and knowledge of human nature. Scientific knowledge alone avails nothing, and the most learned medical men would be often helpless in such a position.

Young men who occupy these places often become wearied of this form of practice and resign, but, after short experience on shore, go back to this life again, and remain until disabled from rheumatism or other troubles. Most of the surgeons are selected from the other side, and a constant sifting out is going on. The genius and skill of those adapted for the work gradually work up to popular lines, and others fall out or retain their places on less popular ships. Lower down a large class of medical men act as stewards or captain's clerks, as well as surgeons, on steamers to South America and less popular lines of travel. They are able to command from fifty to one hundred dollars a month or more in this way.

Occasionally an inland physician secures a position on some second-class steamer, and, what with sea-sickness and disrespect from the officers, and annoying, exasperating demands for his services, which is entirely different from any past experience, he is only too happy to leave this field for others. To any physician who wants to travel, we advise never to aspire to a surgeon's position, but lay up some money and pay for the passage, and come home rested. For a young man who expects to reach any eminence, such a place is fatal to his ambition. An old gray-headed surgeon on one of the great lines said: "I am nothing but a purveyor of fashion and hotel nurse. I have spent over two-thirds of my life at sea away from my family and the comfort and quiet of home life. All I have to show for it is a small house, a life insurance policy and a few hundred in the bank. I have built up no patronage, except on the ship, which is as transient as the clouds. My acquaintance is so scattered as to be of no value. I have no intimate associates, and, although the center of popularity on the ship, I am practically friendless and alone."

There are probably several hundred surgeons that sail in and out of New York, and not one has attained any scientific eminence or made any special contributions that are original to science, or even pointed out any new theories or methods of treatment for sea-sickness. This is not the fault of the men, but of the vicious system which makes them mere clerks. Many of these surgeons are very clever, capable men, who in other surroundings would have attained eminence, but here they fall into a narrow groove, and after a few years are unable to extricate themselves, and so go on through life.—The Cincinnati *Lancet-Clinic.*

THE NATURE OF GONORRHEAL INFLAMMATION.—The character of the inflammation set up by the gonococcus was discussed at length in the congress recently held in Frankfort. In the Centralbl. f. Gynæk. the results of the discussion are given in an article by Wertheim.

Bumm took the stand that in the vast majority of cases of gonorrhea the affection was one of the mucous membrane; that the gonococcus is purely a parasite of the mucous membrane, and, except under certain conditions, which seldom arise, it does not extend into the other tissues; and that, especially in chronic inflammation, it exists in the superficial epithelium.

Neisser expressed the opinion that a gonorrheal inflammation is essentially an epithelial affection, and it is at least questionable whether gonococci are able to penetrate into healthy connective tissue. Wertheim claimed that numerous investigators have proved the ability of gonococci to exist in the cutaneous tissues, and that in every case of gonorrhea in a mucous membrane a sufficiently careful investigation will demonstrate their presence in the subepithelial tissue. This fact is so important for the correct understanding of gonorrheal inflammation that it ought never to be lost sight of. It explains the frequent obstinate course of gonorrhea and the frequent failure of treatment. In order to overcome the gonococci, means must be employed whose effects extend to the superficial portion of the subepithelial layer of connective tissue. To do this thoroughly would be to destroy in toto the epithelial layer, and hence the failure of remedies in so many cases.

Certain other ideas formerly held in connection with gonorrhea are now known to be false. We know to-day, for example, that a gonorrheal infection of pavement epithelium is by no means an impossibility. This has been demonstrated to be true of the vagina as well as of the bladder. We know, further, that gonococci can multiply in the endothelium of the peritoneum as well as in the subendothelial connective tissues. We know that the changes which are so often found in gonorrhea of the tubes or ovaries—the adhesions, the infiltrations of the pelvic organs and cellular tissue, the collections of pus in the tubes and ovaries, etc.—are purely of a gonorrheal nature. We know, further, that there is a gonorrheal metritis. Finally, we recognize that there are undoubted gonorrheal metastases; that the gonococci can penetrate into the capillary vessels at the seat of the primary lesions and set up an arthritis or an endocarditis, or even a myocarditis.

The knowledge of these facts rests on demonstrations of the gonococci, and must be regarded as scientifically exact. There is no reason to suppose that these distant lesions, except in isolated cases, are due to a mixed infection. This supposition rests on the exploded theory that gonococci can neither penetrate into pavement epithelium nor into the peritoneum nor into the deeper layers of the connective tissue.

However, in spite of all these changes in our views of the nature of the gonococcus, Bumm's statement remains true that gonorrhea in the vast majority of cases is an affection of the mucous membrane. It always was that, and that it will always remain; but it is quite another thing to say that the gonococcus is purely a parasite of mucous membrane, and, even if favorable influences are necessary to permit its spread to other tissues, such influences are encountered not so very seldom.—*American Medico-Surgical Bulletin.*

CONTAGIOUSNESS OF CANCER.—Leon Noël has collected together a large amount of information on this subject, more particularly in support of the idea that the origin of cancer should be sought for in some widely-spread condition affecting very various organisms both animal and vegetable. This idea, originated by Fiessinger, is based, first of all, upon the fact that cancer seems to be most frequent in isolated houses on the banks of rivers, especially if close to woods. It has also been observed that trees under these same conditions are affected with veritable tumors which present a curious resemblance to cancer. Among other facts cited by Noël is one which goes to show a certain relation between arboreal "cancer" and that of man; that is, not only the frequency of malignant tumors in habitations surrounded by or near woods, but also a considerable mortality from cancer among certain persons whose occupation obliges them to live in these conditions; thus, excise officers who in certain parts of the country pass a considerable portion of their time in isolated paths through woods, have frequently been observed to suffer from cancer. The statistics of Julliard, Bierry, and Fiessinger all contain considerable numbers of excisemen, and it is now a known fact that country laborers are very predisposed to cancer, and traumatism, scratches from brushes, etc., have been thought to have considerable bearing on the etiology of cancer. Cancer of the lip is said not to exist at Lyons, and all those who go into hospital there to be operated upon for growth in that situation come from the fields. The malignant vegetable tumor is found in woods and orchards. This tumor appears to be contagious, a good number of them being often found in the same neighborhood. Insects, as shown experimentally by Morau, more particularly the large wasps found in woods, seem to have a certain predilection for arboreal "cancer." Not only do they carry infective material from one tree to another, but they also disseminate it into human food. Ruffer's observation is quoted that large numbers of protozoa live on insects in the form of saprophytes, and it is quite easy to understand how insects could carry any infective material, and the question is asked: Is it possible for a human being to develop cancer as the result of such infection? It is also easy to understand how water, especially in the neighborhood of woods, could act as the medium of transport. In point of fact, Feissinger speaks of two different methods in which cancer may be propagated—from food and by the fingers. In the one case, cancer of the alimentary tract will result; in the other, any external portion of the body may be affected. Although these questions are merely hypothetical, they merit further observation.—*British Medical Journal.*

CLASSIFICATION OF TUMORS.

COMPREHENDING MALIGNANT AND NON-MALIGNANT NEW GROWTHS, AND CYSTS.

We republish from the last Revised Nomenclature of the Royal College of Physicians (1896), their Classification of Tumors, as it differs materially from that of the previous revision in common use among English-speaking physicians. Our own Association long ago adopted the Nomenclature, which, we suppose, carries with it the adoption of the subsequent revisions, unless otherwise ordered by the Association.

In deference to the needs of the various registration authorities, a distinction has been made in the general and local tables between malignant and non-malignant new growths. The tumors are, however, here classified, according to their structure and the type of normal tissue they most closely resemble. To facilitate returns under the general tables, the malignant growths are here marked by two asterisks. In certain cases malignant and non-malignant growths are unavoidably associated under one head, as for instance, under Sarcoma; the name is then marked with one asterisk.

Cysts are placed in a separate class, and may be returned severally among the local affections of organs; or among new growths (if connected therewith); or as malformations (if congenital); or as parasites (if parasitic).

CLASSES.

I. Tumors composed of one of the modifications of fully developed connective tissue.

II. Tumors resembling in structure, more or less closely, one of the more complex tissues or organs of the body.

III. Tumors composed of cells of an embryonic type sometimes tending to develop into one of the forms of connective tissue—Sarcoma.

IV. Tumors composed of epithelial cells arranged in the spaces of a stroma of more or less perfectly developed and vascular connective tissue—Carcinoma.

V. Cysts.

Class I.—Tumors composed of one of the modifications of fully developed connective tissue.

(The name of the typical normal tissue is printed first in each case, that of the tumor second.)

1. Adipose Tissue. Lipoma. Fatty Tumor.
    - *a.* Circumscribed.
    - *b.* Diffuse. *Synonym,* Fatty out-growth.
    - *c.* Multiple.
    - *d.* Nævo-lipoma.
    - *e.* Fibro-lipoma.
2. Fibrous Tissue. Fibroma.
    - *a.* Firm or hard fibroma, Desmoid tumor.
      Fibrous epulis.
      Fibrous polypus of nose.

Fibroma of nerves. *Synonym*, False neuroma.
Keloid.
b. Soft or areolar fibroma.
Molluscum fibrosum.
3. CARTILAGE. Chondroma.
a. Ossifying. *Synonym*, Spongy or cancellous exostosis.
b. Non-ossifying.
*Enchondroma (when growing in bone).
Ecchondrosis (when growing from cartilage).
c. Myxo-chondroma (when undergoing mucous softening).
4. BONE. Osteoma.
a. Developing from cartilage.
Cancellous, or spongy exostosis.
b. Development from membrane.
Compact or ivory exostosis.
c. Of teeth.
(1.) Growing from the cement.
Dental exostosis.
Hypertrophy of cement.
(2) Growing from the dentine.
Odontoma.
5. MUCOUS TISSUE. *Myoma.
Mucous polypus.
6. NEUROGLIA. *Glioma.

Class II.—Tumors resembling in structure, more or less closely, one of the more complex tissues or organs of the body.

(The name of the typical tissue or organ is placed first, that of the tumor second.)

1. MUSCLE. Myoma.
a. Unstriped. Leiomyoma.
Fibromyoma. Uterine fibroid. Fibroid tumor of prostrate.
b. Striped. Rhabdomyoma.
*Myosarcoma.
2. NERVES. Neuroma or True neuroma.
a. Medullated.
Plexiform neuroma (in part). Bulbous nerves. *Synonym*, Amputation-neuroma.
b. Non medullated.
3. BLOOD VESSELS. Hæmangioma, Angioma, or Nævus.
a. Capillary. *Synonyms*, Port-wine stain, Mother's mark, Teleangiectasis.
b. Venous. *Synonym*, Erectile tumor, Cavernous nævus.

- *c.* With adipose tissue. Nævo-lipoma.

4. Lymphatic Vessels. Lymphagioma, Lymphatic nævus. Congenital cystic hygroma of neck. Macroglossia.
4. Lymphatic Glands. Lymphadenoma.
   Hodgkin's disease.
6. Papillæ of Skin or Mucous Membrane. Papilloma.
   - *a.* Squamous papilloma, Wart.
   - *b.* Columnar papilloma, Villous growth:
     Of rectum. Of bladder.
7. Secreting Glands. Adenoma.
   - *a.* Racemose adenoma (spaces lined with glandular or spheroidal epithelium).
     - (1.) Resembling normal gland tissue. True adenoma.
     - (2.) With excess of firm fibrous stroma Adeno-fibroma, Adenoid tumor.
     - (3.) With stroma of imperfectly developed fibrous tissue. Adeno-sarcoma.
     - (4.) With stroma containing mucous tissue, Adeno-myxoma.
     - (5.) With large cysts throughout the tumor. Adeno-cysto-sarcoma, Cysto-sarcoma.
       With simple systs. With intra-cystic growths.
   - *b.* Tubular adenoma (spaces in tumor lined with columnar or cubical epithelium).
   - *c.* Adenoma of ductless glands, Thyroid adenoma.
   - *d.* Ovarian adenoma.

Class III.—Tumors composed of cells of an embryonic type sometimes tending to develop into one of the forms of connective tissue.

*SARCOMA:

1. Composed of small round cells.
   - *a.* With homogeneous intercellular substance. Granulation sarcoma. Encephaloid sarcoma.
   - *b.* With reticulate stroma. Lympho sarcoma.
   - *c.* Ossifying sarcoma, Osteoid sarcoma (Osteoid cancer).
   - *d.* Chondro-sarcoma (Malignant chondroma).
   - *e.* Glio-sarcoma.
2. Composed of large round cells.
   With alveolar stroma. Alveolar sarcoma.
3. Composed of oval cells.
   With mucous intercellular substance. Myxosarcoma.
4. Composed of large spindle-cells (Fibro-plastic tumor).
   - *a.* Ossifying.
   - *b.* Chondrifying.

5. Composed of small spindle-cells.
    - *a.* Developing into fibrous tissue. Recurrent fibroid tumor.
    - *b.* Ossifying.
6. Composed of round and spindle cells.
7. Containing giant cells. Myeloid sarcoma.
8. Melanotic sarcoma, Pigmented sarcoma.
9. Plexiform sarcoma, Cylindroma.
10. Psammoma, Brain-sand tumor.

Class IV.—Tumors composed of epithelial cells arranged in the spaces of a stroma of more or less perfectly developed and vascular connective tissue.

**CARCINOMA OR TRUE CANCER.

1. Glandular or Spheroidal Carcinoma.
    - *a.* With little stroma and abundant vessels. Encephaloid cancer.
    - *b.* With dense fibrous stroma. Scirrhus or hard cancer. Variety: Atrophic cicatricial scirrhus.
    - *c.* With colloid degeneration of cells. Colloid cancer (Reticular or alveolar cancer) in part.
    - *d.* With myxomatous stroma. Carcinoma myxomatodes.
2. Squamous Carcinoma. *Synonyms,* Epithelioma, Squamous epithelioma.

    Note.—Chimney-sweeper's cancer is to be returned under Squamous Carcinoma.
3. Rodent cancer or Rodent ulcer.
4. Columnar Carcinoma. *Synonyms,* Columnar epithelioma, Adenoid cancer.
    - *a.* With colloid degeneration of cells. Colloid cancer in part.
    - *b.* Arising in ducts. Duct cancer.
5. Thyroid Carcinoma. Resembling in structure the Thyroid gland.

Class V.—Cysts.

1. CYSTS ARISING FROM THE DISTENSION OF PRE-EXISTING SPACES.
    - *a. Spaces lined with secreting epithelium, Retention cysts.*
        - (1.) Acinous or glandular cyst of breast, pancreas, and other glands.
        - (2.) Duct cyst of the breast, pancreas, testicle and other glands.
        - (3.) Calactocele.
        - (4.) Spermatocele.
        - (5.) Mucous cyst. Ranula. Labial cyst.
        - (6.) Sebaceous or atheromatous cyst, Wen.

(7.) Ovarian cyst from Graafian follicles.
- *b. Spaces lined with flattened endothelium. Distension or exudation cysts, Lymphatic cysts.*
    - *a.* Bursal cysts.
    - *b.* Ganglion.
    - *c.* Serous cysts.

2. CYSTS IN WHICH THE FLUID IS CONTAINED IN A SPACE OF NEW FORMATION.
    - *a.* Blood cyst. Apoplectic cyst in brain.
    - *b.* Degeneration cyst. In brain. In tumors.
3. CYSTS OF CONGENITAL ORIGIN.
    - *a.* Inclusion-cyst. Dermoid cyst.
    - *b.* Cysts arising in the remains of fœtal structures.
        - *a.* Unobliterated branchial clefts.
            Deep cysts of neck.
        - *b.* Unobliterated canal of His.
            Sub-lingual cysts in part.
            Sub-hyoid cyst in part.
            Cysts in front of larynx.
        - *c.* Remains of Wolffian body.
            Encysted hydrocele in male (in part).
            Cysts of the broad ligament in female (in part).
        - *d.* Partially unobliterated processus vaginalis testis,
            Encysted hydrocele of cord.
        - *e.* Partially unobliterated canal of Nuck.
            Hydrocele of canal of Nuck.
        - *f.* Cysts of post-anal gut.
4. PARASITIC CYSTS.

Journal of the American Medical Association.

CEREBELLAR FUNCTIONS.

Notwithstanding the expanding limits of neuro physiology our knowledge of the functions of the cerebellum is still very limited. Recently Dr. Max Lowenthal and Mr. Victor Horsley have made some interesting experiments on the relations between the cerebellar and other centers, especially with reference to the action of antagonistic muscles. These experiments were undertaken because of an observation that when both cerebral hemispheres were removed, and as a result active extension tonus of the limbs was obtained, faradic excitation of the upper surface of the cerebellum caused immediate relaxation of such tonus so long as the current was applied, but as soon as the latter was shut off the tonus was immediately re-established. Such an observation, indicating as it did a distinct connection between the cerebellum and the lower centers, made careful examination necessary, and it was found that such an effect was constant provided the tonus was well established; and the effect was not merely an actual relaxation of the triceps, but also a marked contraction of the biceps. Experiments were arranged in which the effect could be graphically recorded, and in the dog and cat it was found that excitation of the left motor cortex and of the left corona radiata produced invariably symmetrical and synchronous contraction of the prepared muscles, biceps and triceps of the right side. Extraction of the crusta on the cut surface before the general tonus was established produced invariably contraction of both biceps and triceps; excitation after it was established of the left crusta produced in thirteen cases contraction of the biceps and triceps, in two contraction of the biceps and relaxation of the triceps, and in one relaxation of the biceps and contraction of the triceps. Excitation of the cerebellum produced very constant effects. After the tonus established by removal of the cerebellar hemispheres was present excitation of the cerebellum caused a striking and active relaxation of the triceps and an active contraction of the biceps on the same side as that stimulated. In twenty-nine experiments it was found that the muscles of the right arm could be influenced by excitation of both halves of the cerebellum, but the effect was greater on the same side as that stimulated. In five experiments excitation of the left half of the cerebellum evoked relaxation of the biceps on the opposite side. When the tonus produced by removing the cerebral hemispheres was not yet established excitation of the surface of the cerebellum constantly produced atonic contraction of either the triceps or biceps, or of both together. Synchronous excitation of the cerebral area for the forelimb and of the cerebellar focus produced, in cases in which the cerebellum is definitely excitable, an addition to the tonus previously elicited from the cortex cerebri. The inquiry is very interesting and seems likely to throw light upon some of the functions of the cerebellum hitherto so obscure.—*The Lancet.*

RESECTION OF THE CERVICAL SYMPATHETIC.

Two cases of resection of the cervical sympathetic for exophthalmic goiter have just been published by two surgeons, and each operation has resulted in complete success. The first case was reported to the Academy of Medicine by M. Réclus, and the operation was performed by a young surgeon, M. Faure. The patient was a woman, aged 21 years, who developed exophthalmic goiter after her third pregnancy. She suffered from salivation, polyuria, crises of diarrhœa, marked tremblings, sensations of heat and cold, feebleness of the legs, very marked exophthalmos, and tachycardia. Electrical treatment brought about some amelioration of the symptoms, but soon all the phenomena reappeared. The eyelids failed to cover the eyes and corneal ulceration commenced; the thyroid was enormously hypertrophied, the apex-beat so markedly as to raise the chest-wall, the pulse-rate increased from 120 to 130, and her hair began to fall off. At length the presence of tuberculous masses in the neck and chest was clearly to be made out. M. Faure accordingly performed the following operation after the method laid down by M. Jaboulay, of Lyons. An incision twelve centimeters long was made on the right side of the next just behind the sterno-mastoid muscle, the cervical branch of the spinal accessory was divided and the superficial and deep cervical plexus came into view. The vascular and nervous structures lying in front of the vertebral column were separated and pulled forward, and behind them the great sympathetic came into view. The superior cervical ganglion was picked up and the afferent branches as far as the middle ganglion removed. Then some seven centimeters of the great sympathetic were resected. A similar operation, but complicated by the removal of some tuberculous glands, was performed upon the left side. During the operation there was no change noticeable in the pulse or in the condition of the thyroid, but by the evening the pulse was less rapid and the patient slept. On the seventh day after the operation the exophthalmos was much less, and, the patient being able to close her eyes, the corneal ulcers began to heal. The circumference of the neck had diminished from 39 centimeters to 36; the pulse-rate had fallen to 90; and the tremors, the salivation, and the diarrhœa disappeared. The second case was reported to the Academy of Medicine at the next meeting by M. Gerard Marchant. The patient was a woman with a neurotic family history. In March, 1896, exophthalmos appeared after a pregnancy, in August thyroid hypertrophy commenced, and in January, 1897, tremors. The exophthalmos was extreme and the eyelids did not cover the eyeballs. The goiter was specially marked on the right side. The pulse-rate was 80, while the patient was at rest, and became 100 on moving. The heart was normal, and at this time the tremors were well marked. M. Marchant then operated. An incision was made in the line of the sterno-mastoid muscle, the various vascular and nervous structures were lifted up, and the great sympathetic was found lying on the deep muscles of the neck. To make certain, the nerve was exposed as far as the superior cervical ganglion. Four centimeters of the nerve were then resected from above the superior ganglion to below the level of the middle ganglion, which is often absent. The nerve was cut on each side successively. The operator began on the left side, and a subconjunctival ecchymosis appeared in the right eye, due, probably, as in the experiments of Dastre and Morat, to excitation of the central end of the nerve by the forceps. After the operation the pupils contracted, the eyeballs diminished, and the goiter diasappeared by about the twelfth day. Later, some of the phenomena returned in a much less marked form, but still obvious. As M. Marchant says, there is still much to be learned.—*Paris Corresp. Lancet.*

THE PROPER TIME TO ADMINISTER MEDICINES.

Alkalies should be given before food. Iodine and iodides should be given on an empty stomach, when they rapidly diffuse into the blood. If given during digestion, the acids and starch alter and weaken the digestion. Acids, as a rule, should be given between the digestive acts, because the mucous membrane of the stomach is in a favorable condition for the diffusion of the acids into the blood. Acids may be given before food when prescribed to check the excessive formation of the acids of the gastric juice. By giving it before meals the osmosis stomach-ward of the acid-forming materials is checked. Irritating and dangerous drugs, such as the salts of arsenic, copper, zinc and iron, should be given directly after food, except where local conditions require their administration in small doses before food. Oxide and nitrate of silver should be given after the process of digestion is ended; if given during food, chemical reactions destroy or impair their special attributes and defeat the object for which they were prescribed. Metallic salts, especially corrosive sublimate, also tannin and pure alcohol, impair the digestive power of the active principle of the gastric juice, and so should appear in the stomach during its period of inactivity. Malt extracts, cod liver oil, phosphates, etc., should be given with or directly after food, so that they may enter the blood with the products of digestion.—*Med. and Surg. Rep.*

A NEW TEST FOR SEMINAL FLUID.

The recognition of spermatozoa in medico-legal researches is often a matter of considerable difficulty, especially if the material under examination is of old date. Great interest, consequently, attaches to the test for semen devised by Florence, of Lyons, which has recently been reproduced in the Boston Medical and Surgical Journal in conjunction with a confirmatory test due to the ingenuity of Dr. W. F. Whitney. Florence's test comprises the use of a reagent often made use of in testing alkaloids, and is known as the tri-iodide of potassium. The test solution contains 1.65 parts of iodide of potassium, 2.54 parts of pure iodine, 30 parts of distilled water. A drop of this solution placed side by side on a slide with a drop of fluid obtained by moistening the seminal stain results in the formation of a number of characteristic brownish-red, pointed crystals, rhomboidal in shape, and presenting a certain resemblance to hæmin crystals. Dr. Wyatt Johnson, who has carried out some investigations with this test, reports that he has been enabled to obtain the reaction with stains upwards of twelve months old, and not only is the reaction not obtained with other organic secretions, but even the semen of animals gives a much less marked reaction. In the same journal, Dr. W. F. Whitney confirms the value of the test, which, however, gives a precipitate with alkaloids, and he suggests another test, which may be used in conjunction with the foregoing. A drop of fluid obtained from the moistened stain is evaporated on a slide and fixed by heat in the usual way. The resulting film is then stained with eosin and methyl green, when it will be seen that the head of the spermatozoon is stained at the base of a deep green color, while the anterior part and the tail stain red. This staining reaction is well marked only in human semen, so that a possible source of error is obviated.—*Medical Press.*

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NEWS AND NOTES.

The following programme reached us too late for publication in our last issue:

1. "President's Address Carcinoma of the Breast." W. F. Westmoreland, Atlanta, Ga.
2. "Psychology." J. B. Cowan, Tullahoma, Tenn.
3. "The True Physician—His Responsibilities—His Duty to his Profession, and the People." John C. LeGrand, Anniston, Ala.
4. "A Pessimistic and an Optimistic View of Medicine." Y. L. Abernathy, Hill City, Tenn.
5. "A Boquet of Remedial Agencies." John P. Stewart, Attala, Ala.
6. "Treatment of Typhoid Fever." Jno. A. Larrabee, Louisville, Ky.
7. "Abortive Treatment of La Grippe." E. H. Richardson, Atlanta, Ga.
8. "Electro-Therapy in Medicines." Louise Eleanor Smith, Chattanooga.
9. "A New Mode of Internal Electro-Therapy." R. P. Johnson, Chattanooga, Tenn.
10. "Common Sore Throat." James H. Atlee, Chattanooga, Tenn.
11. "Vaccination." Seale Harris, Union Springs, Ala.
12. "The Pathology and Diagnosis of Early Phthisis." Llewellyn P. Barbour, Tullahoma, Tenn.
13. "Sero-Therapy in Tuberculosis." Paul Paquin, St. Louis, Mo.
14. "Importance of Early Recognition of Pleural Effusions, Due to Causes Other than those located in the Pleuræ." Louis H. Jones, Atlanta, Ga.
15. "Abnormal Metabolism." G. W. Drake, Chattanooga, Tenn.

16. "The Subject of Hæmatology." E. C. Anderson, Chattanooga, Tenn.
17. "Inhibition, Physiological and Pathological." J. F. Peavy, Atmore, Ala.
18. "The Rational Treatment of Cancer of the Uterus." Geo. Wiley Broome, St. Louis, Mo.
19. "A Case of Fracture of the Skull, followed by Basilar Hemorrhage; Trephining; Recovery." Curan Pope, Louisville, Ky.
20. "The Relation of the Cause to the Immediate and Remote Results of Fracture. R. M. Cunningham, Birmingham, Ala.
21. "Is Cancer Contagious?" E. Mather, Paterson, N. J.
22. "Stimulants and Narcotics in Obstetrics and Gynecology." R. R. Kime, Atlanta, Ga.
23. "Surgical Shock." Gilbert I. Cullen, Cincinnati, O.
24. "Cystitis; Its Course and Treatment." W. L. Nolen, Chattanooga, Tenn.
25. "Stricture of the Urethra and its Treatment." Wm. R. Blue, Louisville, Ky.
26. "Operative Treatment in Enlarged Prostrate." H. H. Grant, Louisville, Ky.
27. "Treatment of Chancroidal Ulcers." A. R. Danforth, Atlanta, Ga.
28. "Ablation of the Scrotum for Conditions other than Varicocele." W. S. Goldsmith, Atlanta, Ga.
29. "The Treatment of Cancer of the Rectum." J. M. Mathews, Louisville, Ky.
30. "Burns and Scalds." G. A. Baxter, Chattanooga, Tenn.
31. "Metatarsalgia, or Morton's Painful Toe." Geo. S. Brown, Birmingham, Ala.
32. "The Application of Plaster Jacket and Dressing." F. B. Sloan, Cowan, Tenn.
33. "Surgery of the Stomach." H. Berlin, Chattanooga, Tenn.
34. "Report of Operative Cases (Brain)." S. G. Courtney Pinckney, Atlanta, Ga.
35. "Fracture of the Elbow." B. G. Copeland, Birmingham, Ala.
36. "Treatment of Fractured Maxillarie." D. S. Arnold, Atlanta, Ga.
37. 'The After Treatment of Abdominal Operations." Valentine Taliaferro, Atlanta, Ga.
38. "Epiplocele, Report of a Case." J. W. Griggs, West Point, Ga.
39. "Appendicitis." G. Manning Ellis, Chattanooga, Tenn.
40. "The Evolution of the Treatment of the Stump, in Operations for Appendicitis." W. D. Haggard, Jr., Nashville, Tenn.
41. "Obstetrics." W. G. Bogart, Chattanooga, Tenn.

42. "Some of the Mammoth Ovarian Tumores of Surgical History." A. M. Cartledge, Louisville, Ky.
43. "Cases of Ectopic Gestation, Operated on by the Vaginal Route." W. E. B. Davis, Birmingham, Ala.
44. "The Technique in Hysterectomy for Uterine Myomata." W. H. Wathen, Louisville, Ky.
45. "Hysterectomy." Louis Frank, M. D., Louisville, Ky.
46. "Hysterectomy in the Treatment of Pelvic Diseases." Geo. R. West, Chattanooga, Tenn.
47. "Flap Operation for Artresia Vaginæ." Geo. H. Noble, Atlanta, Ga.
48. "Meningitis in Children." J. W. Russey, Chattanooga, Tenn.
49. "Mouth Breathing in Children and its Dangers." Wm. Cheatham, Louisville, Ky.
50. "Injurious Results of Incompetent Refraction Work." Frank Sims, Atlanta, Ga.
51. "The Causes, Diagnosis, and Prognosis, of Valvular Diseases." Hazle Padgett, Columbia, Tenn.
52. "Rheumatism as an Etiological Factor in Cardiac Diseases." S. W. Fain, Chattanooga, Tenn.
53. ....Hunter McGuire, Richmond, Va.
54. ....J. R. Wells, Stone Mountain, Ga.
55. ....Louis McMurtry, Louisville, Ky.
56. ....C. A. L. Reed, Cincinnati, O.
57. ....Wm. Warren Potter, Buffalo, N. Y.
58. ....Geo. Ben Johnson, Richmond, Va.
59. ....Joseph Price, Philadelphia, Pa.
60. ....Joseph A. White, Richmond, Va.

We congratulate the Tri-State Society on its excellent array of papers.

ALVARENGA PRIZE OF THE COLLEGE OF PHYSICIANS OF PHILADELPHIA.

The College of Physicians of Philadelphia announces that the next award of the Alvarenga Prize, being the income for one year of the bequest of the late Señor Alvarenga, and amounting to about one hundred and eighty dollars, will be made on July 14, 1898, provided that an essay deemed by the Committee of Award to be worthy of the prize shall have been offered.

Essays intended for competition may be upon any subject in medicine, but cannot have been published, and must be received by the secretary of the college on or before May 1, 1898.

Each essay must be sent without signature, but must be plainly marked with a motto and be accompanied by a sealed envelope having on its outside the motto of the paper and within the name and address of the author.

It is a condition of competition that the successful essay or a copy of it shall remain in possession of the college; other essays will be returned upon application within three months after the award.

The Alvarenga Prize for 1897 has been awarded to Dr. Joseph Collins, of New York, for his essay entitled "Aphasia."

Thomas R. Neilson, Secretary.

COLLECTIVE INVESTIGATION OF INFANTILE SCURVY.

The American Pediatric Society proposes to make an investigation of as many cases as possible of infantile scurvy. These collective researches have proven of great value and any member of the I. A. R. S. who knows of any cases will greatly assist the undertaking by sending in a report. The society's notice is as follows:

The American Pediatric Society is making a collective investigation of infantile scurvy as occurring in North America, and earnestly requests the coöperation of physicians, through their sending of reports of cases, whether these have already been published or not. No case will be used in such a way as to interfere with its subsequent publication by the observer. Blanks containing questions to be filled out will be furnished on application to any one of the committee. A final printed report of the investigation will be sent to those furnishing cases.

J. P. Crozer Griffith, M. D., Chairman, 123 S. 18th street, Philadelphia.
William D. Booker, M. D., 853 Park avenue, Baltimore.
Charles G. Jennings, M. D., 457 Jefferson avenue, Detroit.
Augustus Caille, M. D., 753 Madison avenue, New York City.
J. Lovett Morse, M. D., 317 Marlboro street, Boston. Committee.

The Water Supply of Boston.—The State of Massachusetts has undertaken to supply the metropolitan water district, including Boston and twenty-seven other towns and cities, with pure water from an immense lake, nine miles in length, covering forty-one hundred and ninety-five acres, three hundred and eighty-five feet above high-water mark, and with an average depth of forty-six feet. It is to take the place of what is now a busy manufacturing district in the valley of the upper Nashua River. The greater part of two towns and five villages, including churches, schools, the houses of seventeen hundred and eleven people, two large mills, and hundreds of small farms will be wiped out of existence to provide for this enormous reservoir. Its construction made necessary the passage of a law, said to be without a precedent in this country, which provides that the State shall pay to such employees of the mills at the time of the confiscation of the property a sum equal to six months' wages. The State also treats liberally people with established businesses, the value of which the seizure of land for reservoir uses completely destroyed, by a liberal allowance for accumulative or prospective damages. When all this is done, the people of Massachusetts will have a bill variously estimated at from $30,000,000 to $50,000,000 to pay.—*The Sanitary Era.*

A Prize for the Discovery of the Bacillus of Yellow Fever.—A bill has been introduced into the Legislature of Brazil offering a prize of $220,000 to be divided into two equal parts, which are to be awarded to the author of a work demonstrating the existence of a bacillus of yellow fever, and the method of recognizing it, and to the discoverer of an effective treatment of the disease. The Medical Institute of Rio Janeiro, the Hygienic Institute of Berlin, and the Pasteur Institute of Paris are to decide upon the award. The bill further provides for the reservation of a sum of $110,000 to found an establishment for the preparation of a curative serum, the discoverer of which shall be appointed organizing director of the institute. The first of these prizes will probably be awarded to Dr. Sanarelli, an account of whose work was given in our issues of July 22 and 29.—*Boston Med. and Surg. Journal.*

REVIEWS.

The Essentials of Obstetrics. By Charles Jewett, M.D., Professor of Obstetrics in the Long Island College Hospital, Brooklyn, New York. In one handsome 12mo. volume of 356 pages, with 78 illustrations and 3 colored plates. Cloth, $2.25. Lea Brothers & Co., Publishers, New York and Philadelphia, 1897.

The author is not only a well-known practical man but a teacher of no inconsiderable experience, and the methodical, systematic way in which he presents this subject must commend his work not only to his pupils but to the profession at large. So often a practitioner wishes to get at the facts without struggling through a whole volume; and that is exactly what he finds in the work before us. See a good illustration of his method. Under "Laceration of the Passages" he considers, under "Treatment," (a) Time for Repair, (b) Suture Material, (c) Needle, (d) Anæsthesia, (e) Operation, and under the last named he says with great good sense, "the aim should be to restore accurately the normal relations of the parts." The details of "artificial feeding and infant dietary" are given in a useful way that will challenge competition with many of the larger works and are themselves worth the price of the volume.

Differential Diagnosis and Treatment of Coma. Embracing Points in Lectures of—and Revised by—Prof. A. P. Grinnell, M.D., University of Vermont. Arranged by George A. Huntley.

This is a little chart which brings out in parallel columns the essential differential points in the several forms of coma. To an undergraduate student it must be as convenient as it is perplexing: it systematizes the accounts in the text-books and emphasizes the salient points; but it would not be safe to take this or any other chart as an infallible guide in coma. For instance: Under Uræmic Coma, we find that there is "general œdema" of the body—often not the case; that there is "no diarrhœa" (vomiting not mentioned), though this is by no means uncommon; and the condition of the pupils are not wasted. But the chart is useful for reference and comparison, if for nothing else to show how much the individual cases will differ from the lines laid down for them by the text-books.

GAILLARD'S MEDICAL JOURNAL.

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Scientia et Veritas Sine Timore.

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EDITORIALS.

ARE MINERAL WATERS ABSORBED BY THE SKIN?

The commercial spirit which dominates most health resorts, especially those in which mineral springs from the chief attraction, has so long obscured the real therapeutic points involved, that physicians and lay people alike have come to regard mineral springs as possessing some mythical power not easily explained. Nevertheless the proprietors and unfortunately some local physicians point with pride to the many cures resulting from the application of their special treatment.

That the internal use of mineral water produces positive physiological and therapeutic effects cannot be denied, nor is there any doubt that chemistry has not yet succeeded in imitating them with sufficient precision to afford analogous therapeutic effects. This may be due to the fact that the latter is not dependent upon the chemical ingredients alone,—indeed these play but a minor role when compared with other elements. The large quantity of water usually consumed at these springs, its temperature, its CO_2-contents, the capacity for absorption of the mineral ingredients, all these come into play in the consideration of their therapeutic effects.

It is a mistake to regard the various gases contained in mineral water, except CO^2 as exerting any decided effect upon the system; their quantity is too small and their combination too fickle to be of much service. It does seem opposed to common sense to regard the infinitesimal quantity of bromine and iodine, iron, etc., contained in many mineral waters as effective therapeutic agents. What, then, is the real source to which the improvement in the conditions of patients sent to these springs may be attributed? The writer has said elsewhere (Hare's American System of Therapeutics, Vol. I.) and he does not hesitate to insist here that the excellent results derived from the removal of a patient to these mineral spring resorts arise from the exhilaration due to improved hygienic, atmospheric and scenic conditions under which the patient is placed. Add to this the freedom from care and business pressure, the stimulus of diversion and amusement, regularity of habits, strict diet and regime, the drinking of unwonted large quantities of fluid, which sweeps out effete material, and there is little left for the mineral ingredients to accomplish. While therefore a positive effect cannot be denied to the imbibition of natural mineral water, there is good and sufficient ground to doubt any such effect from the absorption of the mineral ingredients applied by bathing in the natural mineral waters.

Many of our readers will be inclined to be skeptical upon this point, so firmly rooted is the idea in the mind of the profession that mineral baths produce their effect by reason of their chemical constituents chiefly. Fortunately we have sufficiently positive experiments to decide this question to remove any doubt that may exist. Among many of these observations these of Stas (La Presse Med. Belge, 1886, No. 13), may be mentioned. Stas subjected himself on three successive days to baths of 86° to 90°, containing 50 mgm. of arsenate of soda to the litre. The skin was softly stroked and the urethra closed before the bath. Although he remained in this bath for prolonged periods, not the slightest absorption was noted. The same result was obtained from baths of iodide of potassium. Another observer, R. Winternitz, has found the shaved skin of rabbits to take up slightly a 1½ per cent. watery solution of strychnine, while the human skin failed to take up atropia, cocaine and aconitin dissolved in water, although a very minute quantity was absorbed when dissolved in ether. The only recent experiment which demonstrates the contrary is that of Kopff (Virchow-Hirsch Jabhrb, 1887) who claims to have after four baths in a corrosive sublimate solution and footbaths of iodide potash solution, the enormous quantity of 0.00418 iodine is all the urine passed during the four days.

Dr. Menil concludes correctly from his experiments that the human skin is incapable of absorbing watery solution of ordinary substances even after long maceration, but that it is capable of absorbing 1% solution of the so-called keratolytic substance, carbolic salicylic acid and salol, through its undamaged surface. Since no mineral springs contain any of these keratolytic ingredients, it may be safely concluded that no mineral bath is capable of inducing absorption of any of its chemical elements. This negative result does not, however, militate against the general impression that such mineral baths are exceedingly useful and effective therapeutically. On the contrary, we have here, as in the drinking of mineral waters, the same suggestive climatic and hygienic elements which are so effective in the latter.

Just as the imbibition of mineral water, strongly impregnated with purgative or other salts, offers immediate evidence of their imbibition, there is in the temperature effect of baths similar evidence of action. The heat or cold conveyed by the peripheral, cutaneous nerves to the central nervous system, and thence reflected through the motor tracts, are the really effective elements in the mineral baths. The latter are aided by only such ingredients as stimulate the cutaneous nerves, e. g., strong saline or CO^2 constituents. Other mineral ingredients are indifferent in their effect upon the skin, and as has been shown utterly incapable of entering the system by cutaneous imbibition.

These are facts substantiated by exact experimentation—facts which must unsettle long-cherished ideas and arouse a healthy skepticism in the minds of medical men.

A NEW EPOCH IN PATHOLOGY.

The recent meeting of the New York State Medical Society was prolific in excellent papers. This association has developed with such remarkable vigor, that the unfavorable prognostications made by many at its inception have been falsified by its record.

Among the excellent essays presented at the last meeting, a paper by Dr. Van Giesen deserves special mention, because it offers a practical basis for bringing order out of the chaos now existing in our knowledge of nervous diseases. Dr. Van Giesen insisted upon four cardinal factors upon which a really new pathology of the nervous system may be constructed, in that the nerve fibre is not distinct from, but the outgrowth of the nerve cell; that many obscure phenomena have been cleared up by the linking of physiological chemistry with pathological anatomy; the recently acquired knowledge that the cell is itself a microcosm; and the comparison of the pathology of the nerve cells with that of other parts of the body.

Dr. Van Gieson insists, with much force and logical acumen, that hitherto the nervous system has been regarded, pathologically, as a separate part of the body, and that this false conception was not removed because of the hardening of sections by Mueller fluid, which destroyed all opportunity for studying the minute structure of the cell. Dr. Van Gieson insists that we should regard pathologic conditions in the nervous system in the same light as those in the kidneys or liver, and it is important to realize that, when deleterious agencies come in contact with the nerve cell, the same result ensues, as has been noted under the same conditions in the cells of the kidneys, liver, etc.

Under the simplified study, he claims to be justified in reducing the pathologic condition affecting the nervous system to degeneration, inflammation and necrosis; and that all diseases could be traced to one of these conditions acting with varying intensity.

The most important practical point in this paper, however, is the elevation into its true importance of the resisting forces of the body into a prominent factor in the consideration of disease.

That the condition of the human organism, at the time of exposure to an etiological factor, is a paramount element in the development of disease has been a slowly but surely growing idea in the medical mind. The serum therapy of diphtheria has given us an insight into the probable source of that resisting capacity which protects the organism against the invader by showing how the latter may be met and conquered. This seems to be the narrow and tortuous path by which the great highway to grand therapeutic development may be reached. To strive for this noble goal is, indeed, worthy of all the brave and painstaking efforts now being made in the laboratories and clinics of the world.

Applying this idea to the diseases of the nervous system, how portentous seems the future?

Dr. Van Gieson does not indulge in speculation, however, but with an eye single to the development of his subject, points out that the structure of the brain may be compared to that of the kidney, the parenchyma representing the nerve cells; the stroma representing the neuroglia. When there was disturbance of the kidney, a lack of blood supply, too much blood, the presence of toxins, cloudy degeneration ensued, the cells were destroyed, or not, according to the intensity of the etiological factor. The brain suffers similar effects from similar causes, causing, when evanescent and mild, the delirium of fevers, for instance, and, when persistent, causes insanity. The prognosis is affected by the resisting capacity of the body quite as much as by the character of the toxin. A small amount of poison acting for a long time would make repair less hopeful. In the acute infectious conditions the cause is neutralized by the vis medicatrix in the chronic condition,as in syphilis and locomotor ataxia,the nerve cells undergo degeneration and destruction, and are replaced by other tissue, which destroys the function of that particular part.

Our readers will do well to carefully study the classical paper of Dr. Van Gieson.

GAILLARD'S MEDICAL JOURNAL.

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ORIGINAL ARTICLES.

ABSTRACT OF PAPER ON SURGERY OF THE GALL-BLADDER AND DUCTS.*

By J. McFADDEN GASTON, M.D., Atlanta, Ga.

A division of the bile channels connected with the liver, into the biliary tubules, the hepatic, cystic and common ducts is generally recognized by surgeons.

The first named collect the bile from the acini in the respective lobes of the liver and carry it through two channels which are included with the hepatic arteries and portal sinuses by the capsule of Glisson in the transverse fissure. These branches are united to form the hepatic duct, which meets the cystic duct at an inclination varying slightly from a right angle, and they coalesce in the common bile duct, which enters by an oblique opening into the duodenum about two inches from the pyloric orifice of the stomach.

The gall-bladder, being a temporary reservoir for the bile, is a pear-shaped sac about three inches in length, which is attached to the under surface of the right lobe of the liver, with its globular fundus corresponding to the anterior margin of that organ, and its neck directed obliquely backward, terminating in the cystic duct. While this serves as a diverticulum of the hepatic secretion, a portion of this fluid is conveyed directly through the ductus communis into the duodenum. The passage of the chyme over the papilla at its outlet serves as a stimulus for the discharge of the bile accumulated in the gall-bladder, so that this becomes mingled with the alimentary mass in the duodenum.

It is found that the bile collects in the gall-bladder during the intervals of digestion; and its evacuation may be due in part to the mechanical pressure of the distended stomach against the sac on the under surface of the liver.

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*Presented to the Southern Surgical and Gynæcological Association, at its meeting on November 9, 1897, in St. Louis, Mo.

In the normal state, bile is not found in the stomach, and any regurgitation of it into that organ interferes with digestion, but when commingled with the pancreatic secretion in the duodenum a salutary influence is exerted in the assimilation of the food.

Notwithstanding the elaborate investigations upon the functions of the liver, and the rôle of the bile in the digestive processes, there is still great discrepancy in the opinions of physiologists and clinical observers as to the office of this product in the intestinal canal.

The painstaking experiments of Legg as to the province of the bile in the processes of digestion led him to the conclusion that "the only office which remains to it is that of emulsifying fats and of changing starch into sugar."

But this inference from experimentation is neutralized to a great extent by the weight of clinical experience in regard to its important agency in intestinal digestion.

The study of biliary derangements by observing clinical phenomena contributes more to a proper interpretation of the surgical pathology of the gall-bladder and ducts than the inferences from physiological experiments have done, and it is incumbent upon the investigator to duly weigh not only the effects of retention and diffusion of biliary elements, but the grave consequences of the entire loss of the bile to the physical organization.

Harley claims that "the uses of the bile cannot altogether be dispensed with, for animals with a biliary fistula invariably lose flesh, become emaciated and weak. The hair falls off, the bowels become irregular, while at the same time a great and almost constant discharge of foul-smelling gases takes place from the intestinal canal. At length, after a shorter or longer period, the animal sinks and dies."

In the eminently practical work on the liver by Dr. James Johnson, many important modifications of the system connected with the derangement of the hepatic function are delineated in graphic terms, while it is held that, "the consequences of the want of bile in the alimentary canal are truly momentous."

The able treatise of Murchison sets forth prominently the effects of disordered secretion of the liver, and the serious results upon the general health of any interruption in the supply of bile to the intestinal tract.

Bartholow, in his profound essay upon "Diseases of the Liver," insists upon the importance of bile in nutrition, and the marked influence on nutrition of the absence of bile from the process of digestion in the intestine; while he points out the manner in which this diversion of the bile interferes in the digestion of certain substances.

It is not improbable, however, that in some cases of temporary stoppage of the duct, either from concretions or hardened mucus, there may be a dislodgment of the obstruction with an evacuation of the contents of the sac. Again there may exist inflammation of the tissues of the duct without adhesion of its walls, and the swelling and turgescence may disappear so that the canal shall be restored to its normal state, and serve to convey the bile to its proper destination in the duodenum. Should these results not come about with medical treatment, it becomes necessary to evacuate the distended gall-bladder by an operation, with a view to establish either an external outlet or by catheterization of the tract from the sac into the duodenal canal.

The subsidence of the inflammation may thus restore the natural discharge, and under such favorable conditions the external opening may be closed, obviating a fistulous outlet. This fortunate result in securing against the waste of bile, relieves this process of a most serious objection as a radical operation.

It is requisite for the successful issue of any operative procedure, that a communication with the alimentary canal shall be established; and the temporary discharge of bile by an external opening, with a view to subsequent restoration of its natural channel and ultimate closure of the fistulous outlet, is not liable to the grave consequences of a permanent waste of the biliary secretion.

When the common bile duct remains impermeable an artificial connection of the gall-bladder with the duodenum has been proposed, as a feasible recourse for returning the bile to its appropriate destination in the alimentary tube. The claim of this cholecysto-duodenal communication to consideration rests chiefly upon the great importance of the bile in the digestive and assimilative processes, during the passage of the food through the upper portion of the small intestine.

The entire removal of the gall-bladder in cases of atrophy or degeneration of its tissues has been resorted to without serious interference with the biliary functions, except that the bile flows directly through the common duct into the duodenum, instead of having a portion reserved in the storehouse by the wayside for future use.

There is, of course, a departure from the normal state in the delivery of bile continuously into the duodenal canal, instead of the physiological proceeding of retention for certain periods in the sac, and then bring poured out as requisite for intestinal digestion. Yet, it is inferred that, under the change of relations in the bile-producing act, the discharge adapts itself to the natural demand for it, and experiments have proven that the ductus communis becomes endowed with a retentive power by the spontaneous contraction of its outlet for certain periods in the intervals of digestion.

The continuous discharge of bile by a direct outlet from the gall-bladder into the duodenum, when the common duct is entirely occluded, certainly fulfils the ends of its production more satisfactorily than the formation of a cutaneous fistula for its escape externally, by which its influence upon the system is completely lost. At least this is the nearest approach to a normal supply of the biliary secretion, which can be provided under the circumstances, and thus relieves all the embarrassments of the situation.

My investigations in regard to the surgical relations of the gall-bladder to obstruction of the ducts, have appeared in the Atlanta Medical and Surgical Journal, for August and October, 1884, and for September and November, 1885, in the Southern Medical Record, of Atlanta, for March, 1885, in the Medical and Surgical Reporter for September, 1885, and in the proceedings of the Medical Association of Georgia for 1886.

But my most elaborate papers have been published in GAILLARD'S JOURNAL for October, 1884, under the title of "Obstruction of the Gall-duct and its Bad Consequences, with Remedial Operation Suggested," and in the second volume of the "Reference Handbook of the Medical Sciences," with the captions of "Cholecystectomy," "Cholecystotomy" and "Duodeno-Cholecystostomy," and the recent supplement with title of "Surgery of the Gall-bladder and Ducts."

Those interested in the surgical treatment of the disorders of the gall-bladder and ducts are referred to the above publications for important data which cannot be presented in this paper, while the last named articles furnish the basis for the operative measures herein presented.

When the common ducts becomes partially or entirely obstructed at its outlet, while the hepatic and cystic ducts are open, there is an accumulation of bile in the gall-bladder. This may lead to a greater or less dilatation of its walls, with inspissation of the fluid contents or a deposition of concretions. Yet there is generally a marked diminution of the secretion of bile following the arrest of its discharge into the duodenum, and eventually the liver ceases to yield any secretion, so that a stasis exists in the bile-producing process.

It is a well-ascertained consequence of an occlusion of the cystic duct for any considerable period, that various abnormal collections in its cavity ensue, such as glairy mucus, pus or sero-purulent exudations. The presence of gall-stones in the ducts or intermixed with the accumulation in the sac, may complicate such cases, and the simple evacution of the contents does not ordinarily afford permanent relief.

The prime question for consideration is the practicability of adopting any measure of treatment which shall restore the function of the liver and preserve the office of the gall-bladder. In case the derangement should prove to be irremediable, excision of the gall-bladder is indicated. Under such conditions the attention of the surgeon may be appropriately directed to the radical operation of cholecystectomy as first practiced by Van Langenbuck and since by other European surgeons.

The dissection of the adherent layers of the sac from the under surface of the liver does not seem to be called for in excision of the gall-bladder. In one of my canine experiments the outer wall of the sac underwent disintegration, while the adherent tissue was still firmly united to the under surface of the liver. Since the separation of this layer from the proper structure of the liver gives more trouble than any other step in the operation, it may be left attached after trimming off all the loose wall of the gall-bladder, and thus materially simplify the surgical procedure in cholecystectomy.

There is, doubtless, a tendency on the part of the advocates for different operations to undervalue the final cause in the respective cases. Excision or extirpation of the gall-bladder is not expected to supply the place of other processes for correcting its disorders, but is a *dernier ressort* in view of the inadequacy of any means for restoring the functions of this organ and for rehabilitation of the surrounding structures.

When the gall-bladder and cystic duct become obnoxious to the adjacent tissues so as to cause local disorders and general derangement of health, it is the legitimate province of surgery to get rid of them forthwith by extirpation. They no longer subserve the ends for which they are provided, and constitute an abnormality in the animal economy. Their removal is, therefore, rational and philosophic, in like manner as the separation of any other offending member from adjacent structures. This principle is generally adopted for the obliteration of cysts, and when degeneration of structure with decomposition of the contents of the sac is found in connection with occlusion of the gall-bladder, it belongs properly to the category of cysts, as it has no longer any of the capacities of a receptacle of the bile. Instead of retaining an offending member to propagate trouble to other parts, it is the dictate of common sense and sound judgment to sever its connection with the body.

This operation is not warranted in cases of temporary obstruction of the cystic duct, which admits of relief by mechanical or other means, excepting when disorganization of the structure of the sac is imminent. In permanent obliteration of the cystic duct, though the structure of the gall-bladder, up to the time of an exploratory operation, be intact, it is reasonable to infer that degeneration will ensue, so that it becomes a legitimate proceeding to excise it.

Cholecystotomy.—Under this designation are included such operative measure for the relief of disorders of the gall-bladder as may be undertaken without excision of its tissues, independent of puncturing or incising the sac through the walls of the abdomen, for the discharge of fluid collections.

Hepatic abscess originating in the organ, or resulting from dysentery, does not fall within the scope of this paper, yet when the biliary channels become affected from obstructions, with the presence of inspissated bile or concretions in the gall-bladder, a consequential abscess may form in the liver, that calls for consideration. Ziegler states that the irritant will first affect the walls of the ducts and their surroundings and set up inflammation there. Stones formed in the gall-bladder and in the ducts themselves are frequently discharged into the intestine through the common duct. But if one of them lodges or lingers in the duct retention of the bile ensues, and this may give rise to dilatation of the ducts and to infiltration of the liver with bile. The liver tissue may thereupon become degenerate or inflamed, while the parts around the impacted stone become also inflamed or even ulcerated. When such an inflammation of the ducts persists for a long time or leads to a persistent retention of the bile, the inflammatory changes extend to other parts of the liver. Brown or yellow granular biliary concretions appear in the interlobular tissue and within the lobules. The liver cells perish at various points, and inflammatory infiltrations appear within the lobules and terminate in abscess or fibroid induration.

Independent of this record of the pathological changes connected with biliary derangement, it has been noted by other trustworthy observers that the closure of the natural outlet of the bile brings many troubles in its train. Most frequently the progress of dilatation in the gall-bladder, with the consequent suppuration and disorganization in the surrounding structures, indicates the serious nature of the disorder in these cases.

While spontaneous perforations do not pertain to this surgical procedure, it is not out of place to refer to an interesting report upon affections of the gall-bladder tending to result in cutaneous biliary fistula, for which I am indebted to Kemper in a reprint from the "Transactions of the Indiana State Medical Society for 1879."

The aim of surgical interference is to arrest the march of the disorder, and to accomplish a cure.

In the absence of fistula and adhesions, when the history, progress of the disease, examination of patient, and above all the presence of a biliary tumor, and exploratory puncture, reveal the presence of biliary calculi, Boeckel recommends cholecystotomy, but he holds that this operation is not otherwise justifiable. On the contrary, it is clear that we arrest the progress of the inflammatory disturbance in the tissues by evacuating the distended gall-bladder. Delay in undertaking the appropriate measure of relief brings disintegration of structure, which undermines the recuperative powers of the system; and this tendency to disorganization may be arrested by an early recognition of the nature of the case and a timely resort to a judicious operative measure. The earliest report on record of a radical operation for relief of the gall-bladder is that of Bobbs, on June 15th, 1867, upon a lady thirty years old. The gall-bladder was emptied of its limpid fluid contents and several solid concretions, the incision in its walls being stitched and the ends cut close, while the sac was returned into the abdominal cavity. Her recovery was rapid and without an untoward symptom.

The practical outlook was not improved by the untoward results of the radical proceedings of Sims and Keen in their cases of dropsy of the gall-bladder. The condition of the ductus choledochus is not reported in the autopsy of Keen's case, but it is inferred from the post-mortem examination in the case of Sims, that obstruction of the common duct did not exist.

Encouragement has been drawn, however, even from these failures for a repetition of similar operations and notices of the steps taken in other cases by various operators are reported in the January number of the American Journal of the Medical Sciences for 1879 by Keen, and in that of October, 1884, by Musser and Keen.

According to Musser there are four pronounced conditions, which are dangerous as sequences of biliary obstruction, and which may be looked upon as indications for operative interference. They are jaundice; a tumor in the right hypochondrium, presumably an enlarged gall-bladder, frequently recurring paroxysmal pain; and symptoms of suppuration.

The causes in general of stenosis in the bile-ducts are considered to be three: 1. Foreign bodies within the ducts (gall-stones, parasites, etc.) 2. Diseases of the walls of the ducts (a congenital; b, adhesive inflammation or cicatrization of ulcers; c, parahepatitis). 3. Tumors, etc., external to the ducts.

The enlargement of the gall-bladder is generally pyriform in shape, or it forms a semi-globular or globular tumor, and is unusually movable, tender, elastic and fluctuating, but may have different qualities, and the intestine is never in front of the growth.

Acupuncture and aspiration in advance of opening the abdomen are advocated by Keen for the diagnosis of affections of the gall-bladder, but the resort to such exploratory punctures are not sustained by general experience. Yet it is well known that other prominent men concur in the propriety and expediency of this course, and Bartholow claims that this diagnosis can be reduced to an absolute certainty, while Harley insists strenuously upon its safety and efficiency. Being satisfied that this preliminary step is not requisite for the practical management of such cases, and that unforseen risks attend the modes of exploration, while safer means of investigation may be relied upon for a correct diagnosis in this class of disorder, I cannot acquiesce in this measure for the detection of gall-stones in the sac, or for the removal of its fluid contents, in preference to an exploratory incision and examination with the finger. While recognizing the preliminary process, exploratory operations are sanctioned by Keen, and if the ascertained conditions warrant further steps, to be followed by cholecystotomy. He inculcates, as a rule, that the incision should be made over the center of the tumor and parallel to the free border of the ribs. The greater facility of access to the tumor overbalances, in his estimation, any less hemorrhage from the linea alba. It should be sufficient for exploration, being made about three inches in length, and enlarged, if need be, afterward. All bleeding should be arrested by hemostatic forceps or catgut ligatures before opening the peritoneum. This being done by incision upon a directory to avoid wounding the sac, two fingers should be used, or, if necessary, the whole hand, to explore the condition of the various abdominal organs and learn the exact nature, attachment, etc., of the tumor; and, so far as possible, the character of its contents. The cystic and common ducts are to be examined to determine the presence and situations of gall-stones with their size, shape, and mobility. If any be found in the ducts, the suggestion of Handfield Jones to endeavor to push them into the duodenum should be tried. This failing, we should endeavor to push them back into the gall-bladder, as this part of the duct has already been dilated by their onward passage.

It is not always practicable to find all the stones at the outset, and those not encountered at the operation have escaped subsequently or have been extracted by the operators. This shows the propriety of exploring the cavity of the gall-bladder and the cystic duct on dressing the wound, during the first week after an operation.

The first case of Parkes is very instructive touching this point, and in his graphic account of it on January 27th, 1885, I was informed that an exploration of the cavity of the gall-bladder on the occasion of the operation failed to find any stones in the bladder. On the second day the dressings were charged with a profuse flow of greenish-colored fluid from fistula. On the fourth day again dressed; while probing the bladder this day he detected and removed two stones. On the seventh day he found and removed five stones, one as large as the end of the finger. None were found subsequently.

He was satisfied that a contraction of the common duct existed, rendering it too small in caliber to carry off the biliary secretion, and that for the time being the fistula would have to be maintained open.

Had this been evident on the occasion of passing the sound, Parkes might doubtless have adopted the suggestion at page 380, in the October number of GAILLARD'S JOURNAL, that "in case any communication with the duodenum exist, however small it may be, dilatation of the ductus choledochus should be attempted."

Notwithstanding the assurance of Parkes that his finger in the abdominal cavity guided the sound through the cystic and common ducts into the duodenum, and the statement of Sims in the autopsy of his case, that "Ganal was able to pass a probe from the gall-bladder through the ductus communis choledochus into the duodenum," I observe at page 100 of the Medical Bulletin for April, 1885, that Bartholow questions the practicability of catheterization and consequent dilatation of these ducts. While serving as an antiseptic, it also promotes the peristaltic action of at page 83 of the Southern Medical Record, for March, 1885, numerous other examinations were made in dogs, presenting no obstacle to the passage of a sound from the gall-bladder through the cystic and common ducts into the duodenum. While a straight metallic probe may not pass readily from the cystic into the common duct, there is no embarrassment in carrying a curved sound or an elastic bougie from the gall-bladder into the duodenum, and hence in cases of partial constriction of either duct, the successive use of graduated sounds or bougies may be relied upon for dilatation of the passages.

With the lights before us, it is clear that an effort should be made to overcome temporary obstructions of the ducts, and if concretions exist which cannot be moved in either direction by pressure from without the canal with the fingers, an effective recourse is found in adopting the expedient of Tait for crushing the masses with padded forceps, and leaving the fragments to be flooded out by the bile. This suits cases not requiring incision of the sac, but he is in favor of establishing a biliary fistula temporarily, and a large finestrated drainage-tube should be inserted, while the whole abdominal wound is securely closed excepting at the opening for the attachment of the incision in the sac. A compress and bandage should be used to obviate any tendency to ventral hernia after the operation.

Keen also regards the creation of a biliary fistula as the better practice, yet he holds that in case a probe could be passed into the duodenum with a reasonable assurance that all stones have been removed, closure of the gall-badder by Gèly's suture or otherwise might be done with propriety.

In comparing the different measures adopted by surgeons for the relief of a distended gall-bladder, we shall find that their relative advantages depend upon the greater or less probability of restoring the bile to the intestines.

Whatever may be the properties of the bile, it would seem to have been designed for a useful purpose by admixture with the chyme soon after leaving the stomach, and in combination with the pancreatic secretion enables the lacteals to take up the nutritive elements of the food. While serving as an antiseptic, it also promotes the peristaltic action of the intestines.

If a stone remain in the cystic duct the sac will be refilled with its own secretion, and if in the common duct by bile; but should all the stones be removed and the ducts be pervious, the fistula will soon close under the use of drainage through the duct into the duodenum.

Should the bile be certainly restored to its natural channel on the occasion of an operation, the closure of the incision in the sac and dropping it into the abdomen, with complete suture of the external wound, are advisable.

Besides the case of Bobbs, in which only one stitch was used to close the incised sac, those of Courvoisier and Bernays, operated on by this method, have proved entirely satisfactory. Even the fatal case of Meredith, in which the incision of the gall-bladder was closed with the continued suture and dropped into the abdominal cavity, proved the feasibility of immediate closure under favorable conditions. In the succinct report of it forwarded to me February 11, 1885, the author states that this patient died in forty-eight hours with suppression of urine. "At the autopsy the incision in the gall-bladder was found securely closed, and quite impervious to fluid."

The case of Dr. Courvoisier died from pneumonia two and a half months subsequent to the operation, and the autopsy showed a perfect union from the suture of the incised wall of the gall-bladder.

There are some cases, in which complications exist, that preclude the completion of an operation which has been undertaken, such as extensive disorganization, the agglutination of tissues or the evidences of malignant disease. Of the last Senn reported an interesting case to me on February 11, 1885, with the remark, that it is sometimes more important to report unfavorable than favorable cases. A parallel case to this has been reported since in Germany, and though, as Senn states, primary cancer of the bile-duct is an exceedingly rare occurrence, scirrhus of the pancreas and pylorus often involves it in the disease.

Some interesting and instructive notices of the great advances made in operative procedures for disorders of the gall-bladder, appeared in the numbers of the Medical News of Philadelphia for December 20, 1884, and February 14, 1885.

When the outlet of the common bile-duct is permanently closed, and the bile finds its way from the hepatic duct through the cystic duct into the gall-bladder, the most philosophic proceeding is to effect a direct passage of the bile from the gall-bladder into the duodenum.

In my first series of experiments upon dogs for testing this union of the gall-bladder and duodenum a circular row of catgut continuous suture attached the serous membranes around an elastic ligature connecting the two walls. This was done under the impression that it was requisite to excite adhesive inflammation, by which the surfaces should become agglutinated. But it was productive of undue disturbance, and in the last case of that series it was observed that sufficient inflammatory action ensued from the insertion of a single stitch of silk thread to unite the walls of the gall-bladder and duodenum.

With the further observation that this loop cut an opening between the two cavities, in case an incision be made into the sac for exploration, it may be closed by Lembert's suture separate from the parietes of the abdomen; and the external wound may then be sutured throughout without any provision for drainage, as the bile passes very soon through the apertures of the thread and ultimately through the fistula.

With a view to diagnosticate distension of the gall-bladder, all tumors connected with the liver should be examined with care. A peculiarly complicated case of laparotomy is reported by Bauer in the St. Louis Medical and Surgical Journal for June, 1886, in the person of a mechanic aged forty.

This case was calculated to mislead the operator, in view of the fact that "immediately below the right hypochondrium there was a tumor obviously connected with the liver, since it moved contemporaneously with that organ. It presented a semi-globular form, about three inches in diameter, was smooth and elastic to the touch, and withal very tender." The author leaves us entirely in doubt as to his preconceived view by saying that although he had formed some diagnostic views on the nature of the tumor, believing that laparotomy is the only trustworthy diagnostic method, he proceeded to operate. (Query—What does an analysis of the facts prove?) [Abscess of Liver.]

A paper read by Keen before the Philadelphia Academy of Surgery November 2, 1885, presents some interesting points bearing upon the relations of the duodenum to the gall-bladder and liver.

An interesting case of fatal obstruction of the ilio-cecal valve by a gall-stone was reported by Dr. Russell H. Johnson, in the Medical News of June 27, 1885. The author states that "the facts of the case warrant the conclusion that the gall-stone under consideration escaped from the gall-bladder by means of a fistulous tract connecting it with the duodenum," and remarks further that "autopsic examinations (especially those made by Murchison) have in many instances shown the presence of these fistulas and lead us to infer that it is not a very unusual occurrence."

A very instructive report to the New York Surgical Society, at its meeting of April 28, 1885, by Dr. Peters, detailing the results of an autopsy, illustrates strikingly this ulcerative tendency from an occlusion of the gall-bladder and the accompanying adhesions surrounding the perforation into the duodenum by which nature provides an outlet from the gall-bladder into the duodenal canal.

It is evident that the slow progress of such cases at the outset, and the gradual concretion of hard masses in the gall-bladder, favor the development of a subacute form of inflammation, which leads to suppuration in the sac and ulceration of adjacent tissues. The inflammatory action which is sometimes set up in the tissues of the gall-bladder seems to point to relief in the direction of the duodenum, and an autopsic observation of the ulcerative process by which the contents of the gall-bladder passed into this canal first directed my attention to the practicability of effecting a direct communication between these two organs.

No measure apart from that of Von Winiwarter had been adopted previously with a view to convey the bile from the gall-bladder into the intestinal canal. In his case the gall-bladder was first united to the colon, and after the lapse of some time he established a fistula between the sac and the small intestine by uniting them with stitches, suturing the intestine to the abdominal wall, and opening the gut on the fifth day, puncturing the opposed surfaces through the incised intestine, and finally closing the latter with sutures. This operation of Winiwarter was intended to remedy the occlusion of the common bile-duct by effecting an artificial communication with the intestine for the passage of the bile, and while it failed to realize the advantages expected from a cholecysto-duodenal outlet, the partial success of this procedure shows the tolerance of similar measures by the respective structures involved. No complications arose in Winiwarter's case from the coils of intestine, the constriction of its lumen, or the regurgitation of its contents into the gall-bladder; and much less are such difficulties likely to occur in the attachment of the sac to the duodenum.

The practical discernment of Harley led him to the conclusion that the triumph of operative surgery would be to establish an artificial fistula between the gall-bladder and the duodenum. For then not only would the pent-up bile be removed, but the disturbance arising from the non-admittance of bile into the intestines likewise to be at the same time overcome. I am not quite sure, he remarks, if, in these days of antiseptic surgery, the operation is not quite practicable; for I can see no reason why the adjacent surfaces of the gall-bladder and duodenum should not be eroded by potassafusa and speedily stitched together.

The principle of cholecysto-duodenal communication seems to have been grasped in this suggestion for creating a fistulous opening; but the process of accomplishing it presents objectionable features, which do not hold against the plan of uniting the two walls by a loop of suture which shall induce an amount of adhesive inflammation sufficient for the union of their outer surfaces, with a certainty of cutting through the adjoining tissues and effecting a passage for the bile from the gall-bladder into the duodenum.

Harley is justly entitled to priority for the publication of this suggestion, "to establish an artificial fistula between the gall-bladder and duodenum," as his book on Diseases of the Liver, which contains it, appeared prior to the publication of my article in GAILLARD'S JOURNAL on "Obstruction of the Gall-duct and Its Bad Consequences, with Remedial Operation Suggested." But it is due to myself to state that his work had not been seen, nor was I aware of his suggestion when my paper was published. In like manner it may be stated that I did not know of the operation of Winiwarter, though it was recorded in a foreign journal in 1882, and published in the October number of the American Journal of the Medical Sciences for 1884.

But, putting aside the question of priority, it is proper to note the difference in a predetermined recourse to the litigation for opening a communication from the gall-bladder into the duodenum, which shall at the same time induce a firm union of their outer surfaces, from an incidental attachment of the sac to the large or small intestine, as done in his case. It must be perceived that his mode of proceeding has quite a dissimilar effect from the operation that was suggested by Harley, or from that process which I adopted afterward for establishing the flow of the bile into the duodenum, which McGraw has indorsed.

The close proximity of the duodenum to the lower and under surface of the distended gall-bladder, with more or less thickening of its wall, affords favorable conditions for this operation; and it is evident that greater tolerance of the textures for surgical interference is presented after the disorders connected with biliary obstruction have diminished the tendency to active or acute inflammation.

A class of operations calls for attention in connection with impaction of biliary calculi in the cystic or common ducts which cannot be dislodged by the means already mentioned. The incision of the wall of the duct by the knife or scissors, constituting this operation for the removal of stones, has in most cases been closed afterward by suture, but when this is impracticable it may be left open and the intervening space should be packed with gauze, leaving the ends protruding at the external wound, to afford drainage and prevent the entrance of bile into the peritoneal cavity.

The various modes of procedure adopted by different operators in effecting cholecystenterostomy include anastomosis of the gall-bladder with the duodenum, the small intestine and the colon, by different processes besides that of union by a stitch of silk thread or the elastic ligature.

The attached surfaces have been sutured, with the cut edges brought together or the division made after nearly completing the circuit. The bone-plates have been employed to approximate the walls of the gall-bladder and intestinal canal, and the Murphy button has latterly been used in most cases for this anastomosis.

There has been a fair measure of success by each of these procedures, and the choice of operators in the future will depend to a considerable extent upon the conditions presented in the individual cases.

SOME PECULIAR EFFECTS OF CHLOROFORM, WITH PREFATORY REMARKS.*

By EUGENE LEE CRUTCHFIELD, M.D., Baltimore, Md.

In the choice of an anæsthetic I invariably give the preference to chloroform. The indefatigable efforts of the advocates of ether to prove its superiority have failed to convince me of any advantage to be derived from its administration. I hold to this opinion notwithstanding the fact that quite a number of the professors whose lectures I attended strongly condemned chloroform and extolled the virtues of sulphuric ether. I do not believe that the latter is safer than chloroform. Many reasons could be given for this view if such were the object of this paper. This point has been ably considered in a discussion of the subject† which Prof. J. J. Chisolm, M.D., LL.D., presented some years ago at a meeting of the Baltimore Academy of Medicine. Like that distinguished gentleman, I believe that any one who is a fit subject for a surgical operation may take chloroform. I have administered it to patients who had mitral regurgitation. During the operation (amputation of two fingers) in one such case the action of the heart became weak, but this condition was easily overcome by the hypodermic injection of nitro-glycerine (gr. 1-100).

I have never seen a death from chloroform narcosis. Twice while I was administering it the patients ceased to breathe, but, the action of the heart remaining good, they were resuscitated by Nélaton's method. It is rather singular that these patients were mother and son. Possibly idiosyncrasy of an hereditary character was accountable for this phenomenon. On other occasions I have seen the pulse become very feeble while the respirations remained good. In such instances the hypodermatic injection of stimulants like whiskey, brandy, nitro-glycerine, digitalis and strophanthus, or the inhalation of nitrite of amyl answers very well. The most narrow escape that I ever witnessed occurred September 22, 1896, while I was giving chloroform for Dr. Alfred Whitehead, M.R.C.S., Lond. Then both heart and respirations seemed to fail completely, but the patient was revived by the employment of Nélaton's and Sylvester's methods and the subcutaneous use of stimulants. This case was reported in the Maryland Medical Journal of November 21, 1896.

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* Read before the Clinical Society of Maryland, May 14, 1897.

† "Chloroform the Best of Anæsthetics," by J. J. Chisolm, M.D.

In order to be as accurate as possible on this subject I keep a record of every time I give an anæsthetic, in which are mentioned all the particulars of the case, with the name of the operator. While pursuing this plan I have an advantage over those men who boastingly say that they have administered chloroform thousands of times, but yet have nothing to substantiate their assertions. Then, too, I make mention only of those cases in which I myself am the administrator. I have no record of those instances (which are numerous) that I have witnessed, on which occasions others have given the anæsthetic, nor of those cases in which I performed the operation, except a few instances when I both gave the anæsthetic and operated without medical assistance.

Ether I have given but three times, and then I did so only because the operator insisted upon its use. Once, however, when giving the anæsthetic for Dr. H. H. Biedler, because of the extremely weak action of the heart, I changed from chloroform to ether. But as a rule, I would never employ it voluntarily, as its action is slow, it causes great irritation of the respiratory passages, it frequently produces alarming cyanosis, and in many ways it is unsatisfactory. Moreover, it is contraindicated when the kidneys are at all damaged, and it is not to be recommended in the very young or the very old. Again, while chloroform may kill the patient on the spot, ether frequently does so later on. These deaths, however, are seldom attributed to their rightful cause (ether), and hence chloroform is unjustly considered the more dangerous of the two. But, while I am a strong advocate for the use of chloroform in preference to ether, candor compels me to acknowledge that there are certain cases of idiosyncrasy in which the inhalation of chloroform produces unpleasant features. This fact, however, constitutes no argument against the employment of chloroform, since there is not a drug in the whole Materia Medica against which the same charge could not be made. A few cases of idiosyncrasy will now be related.

At a meeting of the Baltimore Medical Association, held March 25, 1889, Dr. Thomas A. Ashby, then president of that body, reported the case of a man in whom the inhalation of chloroform produced a convulsion having the characteristic features of puerperal eclampsia. This was the first case of the kind that he had ever seen or heard of. He was unable to explain the occurrence, especially as chloroform so often relieves this affection in women.

In the early part of the year 1886, while a student in this city in the office of Prof. Whitehead, I administered chloroform to a patient upon whom that gentleman operated for the relief of congenital phimosis. The vapors of the anæsthetic caused considerable irritation of my throat and much coughing, for which at the time I was unable to account. It did not occur to me to attribute this effect to the chloroform until the night of February 11, 1889. I was then called to see a man who had a subglenoid dislocation of the humerus. As attempts at reduction caused him much pain, and in order to overcome muscular spasm, I decided to administer chloroform. I did so, very much to the relief of the patient, but with great discomfort to myself, for it produced the same peculiar irritation of the throat with its attendant coughing.

On October 20, 1891, I gave chloroform to an old lady over eighty years of age before sewing up an extensive wound of the scalp. I was again troubled with the same irritation of the throat and coughing. A singular feature in this connection is the fact that chloroform does not always affect me in this manner. Although I had administered it quite a number of times since February 11, 1889, in no instance since that date had it annoyed me in this way until this occasion.

Although I had given chloroform frequently since the last-mentioned instance, I was not again affected thus by its vapors until December 21, 1894. I then administered it for another physician, who operated upon his brother for the cure of an incomplete fistula in ano. I then experienced the same peculiar irritation of the throat, which produced the same amount of coughing.

January 9, 1897, I gave it to an obstetrical patient of mine before applying forceps. Again it caused the same discomfort. Having felt this unpleasantness from chloroform on a sufficient number of occasions to convince me that it is really due to its vapors, I now make it a rule, when administering this anæsthetic, to assume such a position that I shall inhale as little of it as possible. It is singular that chloroform should have this effect, since one of the reasons why it is to be preferred to ether is its freedom from producing irritation of the mucous membrane of the air passages.

Dr. W. F. A. Kemp, at a meeting of the Baltimore Medical Association, held October 12, 1896, related a case in which there was no primary ill effect from chloroform, but a peculiar after-effect. The operation was performed in the morning, but about 10 P. M. the patient had a well marked case of angina pectoris. The pathology of this disease rests in considerable obscurity, but it often occurs in cases in which there is fatty degeneration of the muscular tissue of the heart associated with calcareous and atheromatous changes in the blood vessels. In these conditions it is said to be unsafe to give chloroform. Thus, we can see that this anæsthetic, which has a paralyzing effect upon the heart, could readily cause angina pectoris if the above-mentioned conditions existed. It is somewhat surprising that in Dr. Kemp's case the effect should have been so long delayed. At any rate, it is a significant fact that two of the most useful drugs in the treatment of angina pectoris, viz., amyl nitrate and nitro-glycerine, are also those of great service in combating the dangerous consequences of chloroform narcosis.

I am indebted to Dr. Percy Stansbury, of this city, for the following case: In the Spring of 1895 he administered chloroform for another physician who was about to remove a congenitally elongated prepuce. The patient, under the inhalation of the vapors of this anæsthetic, became thoroughly relaxed in every respect but one. The conjuntivæ could be touched without the man's flinching, and the arm when elevated and allowed to fall was perfectly limp, but there was persistent priapism. This lasted until the effects of the chloroform had subsided.

The experiment was tried several times, but always with the same result. Finally, ether had to be employed before the operation could be performed. This case is unique in that the erection persisted beyond the stage of excitement. That the nervi erigentes are sometimes stimulated during this stage of chloroform narcosis, has been noticed with comparative frequency, but this effect passes off when the system becomes thoroughly relaxed. Therefore, in this particular, this case possesses peculiar interest.*

From "The Medical Bulletin" (Philadelphia), of November, 1896, I copy the following "Peculiar Case of Anæsthesia:" "Under this title Dr. J. O. Williams, of Houston, Texas, describes in the Southwestern Medical Record the case of a woman who underwent the operation of laparotomy under chloroform. After the operation she proceeded to tell the subject of the conversation and the names of the persons (those whom she knew) who took part, what each one said, the calls for instruments, the dropping of an instrument upon the floor, the call for a certain ligature and the announcement of the same being out, a substitute decided upon. The remarks relative to condition, extent and character of growth, in some instances required technical phraseology, all of which was unintelligible to the patient. In fact, a very succinct account was rendered of the happenings during the anæsthetic state. Thus there was anæsthesia of the tissues of sensation and motion while the intellectual faculties remained impressive, receptive, and finally communicative."

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*Since the above was written Dr. John C. Morfit, of this city, has told me that he once had a similar experience. In his case the operation (circumcision) was performed under chloroform, notwithstanding the persistent priapism.

The following case was related to me by Prof. George Reuling, M.D., the eminent oculist and aurist of this city: Mrs. W., from Georgia, aged about 26 years, was operated upon by him for the removal of a cataract. She was possessed of a highly-strung, delicate, nervous temperament. During the anæsthetic state there was complete abolition of sensation and motility, while ideation was retained and even exaggerated. She conversed with him during the operation, but in such a manner as to indicate an hysterical tension of nervous force. Her sensitive, nervous organization then manifested itself to its full degree. Thus exercising the intellectual faculties to an augmented extent, although feeling no pain and deprived of the power of voluntary motion, she reminded him of one in the hypnotic condition.

In conclusion, I must acknowledge having presented only that which is interesting, but not practical, so far as we can see. And yet, so limited is our vision and its field of operation that we know not what important principles may possibly be embodied in what is here related. It may be that a seed has been sown which nourished in the fertile brain of some thoughtful investigator, shall germinate and spring up and yield fruit that shall be of lasting benefit to suffering mankind. At any rate, these cases reveal to us some of the possibilities that we may witness in the administration of this anæsthetic. They are now given as my humble contribution along this line with the hope that they may awaken in others a spirit of inquiry, and that from them principles may be formulated that shall prove to be of enduring utility.

1232 East Preston street.

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ADDRESS.

ON THE TEACHING OF SURGERY.*

By CHRISTOPHER HEATH, F.R.C.S.,

Holme Professor of Clinical Surgery in University College, London, and Senior Surgeon of University College Hospital.

Gentlemen:—In taking the chair at this, the opening meeting of the Section of Surgery, allow me to express my sense of the high honor I enjoy in presiding here to-day. It is no small matter for an English surgeon to be called upon to preside over colleagues of such eminence as are represented by the vice-president of the Surgical Section, and I beg leave to tender to them, and to the eminent surgeon who is president of the Association, my thanks for having selected me for so distinguished a position. As an English surgeon it gives me the greatest pleasure to meet the members of the profession in the Canadian portion of Great Britain, and also those medical brethren from the United States who have been good enough to attend this meeting. I trust your deliberations will not merely advance the science of surgery, but will cement those bonds of fellowship between the members of a united profession which our common Anglo-Saxon origin should foster and maintain.

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* Presidential Address delivered before the Section of Surgery of the British Medical Association at Montreal, September 1, 1897.

In addition to the various papers which will be read in the Surgical Section, it has been thought desirable that there shall be held two discussions on questions of surgical interest; and after considerable deliberation it has been decided that these subjects shall be: (1) Appendicitis and its Surgical Treatment; and (2) the Treatment of Cancer of the Rectum, with special reference to the High Operation. The honorary secretaries have arranged with certain eminent surgeons to introduce these subjects, one this morning and the other to-morrow, and I trust that the discussions will be well supported.

Nothing, I venture to think, is more remarkable that the recent progress of abdominal surgery. Twelve years ago I was engaged in editing a "Dictionary of Practical Surgery," and neither appendicitis nor the operation for removal of the rectum, with which Kraske's name is connected, was mentioned in it, although I had the assistance of the leading London hospital surgeons. Kraske's original paper, I may mention, was published in 1885, and is referred to in Ball's work on the rectum, published in 1887, and in most surgical works since that date. The subject of appendicitis, so far as British surgery goes, dates from a paper read before the Royal Medical and Chirurgical Society, in February, 1888, by Mr. Frederick Treves, though the title of it was "Relapsing Typhlitis Treated by Operation." In August of the same year Mr. Treves opened a discussion on the "Surgical Treatment of Typhlitis," at the meeting of the British Medical Association at Leeds, and subsequently published his address with additions and alterations, in the form of a monograph, entitled "The Surgical Treatment of Perityphlitis." I do not know to whom we are indebted for the hybrid term "appendicitis," but it did not appear in the index to the British Medical Journal before 1891.

But it is not merely in these two departments that progress has been made. The surgery of the kidney and of the liver has advanced pari passu with that of the hollow viscera, and the labors of Morris and Robson in England, and of Keen and others on this side of the Atlantic, have done much for the relief of suffering and the prolongation of life.

Looking back over forty years of professional life, nothing surprises me more than the change which has come over the treatment of calculus. In my student days, to see Fergusson cut for stone by the lateral method was to witness an operation as near perfection as was conceivable, and the dexterity and rapidity with which the calculus was extracted were only marred by the frequency with which death from septic causes spoiled the skill of the surgeon. To have one's first lithotomy was an event in the life of the young surgeon, and every now and then a reputation was spoiled by some contretemps in the public performance of the operation. Later I was the frequent witness of my colleague Henry Thompson's skill in using the lithotrite to break up the calculus in a series of "sittings." Then came in "litholapaxy," or rapid lithotrity, which we owe to Bigelow, the great American surgeon; and, lastly, that recurrence to the old high or suprapubic operation which was due to the Scandinavian surgeon Petersen. Hence the student of to-day rarely, if ever, sees a perineal lithotomy, and as a consequence his interest in the anatomy of the parts concerned in the operation has greatly diminished. Possibly the surgeons of the last generation laid too much stress upon anatomical details, but it is somewhat remarkable to find how little anatomy seems to serve for practice in the present day.

I am told by those who are teaching anatomy now that it is difficult to get the student to take the trouble to make a neat dissection, because he can find in the various museums, and notably at the College of Surgeons of England, such beautiful preparations in spirit that he prefers to study from them or from pictures rather than labor to get out the details for himself. If this is so I can only regret that the present race of students is so short-sighted, for without a working knowledge of human anatomy I can conceive of no progress in surgery.

But I regret to find that in Great Britain at least the teaching of anatomy is gradually getting more and more into the hands of professors who are anatomists, but not surgeons, and that their tendency is to lay stress upon transcendental details rather than surgical relations. When these gentlemen happen to become examiners this tendency to specialize becomes very marked, and as this applies equally to the teachers of physiology and chemistry, the unfortunate medical student becomes the victim of science (falsely so called), and sometimes develops into that marvelous being, a London B.Sc.

But seriously, are we not overdoing the scientific teaching of the man who has after all to get his living as a practitioner of medicine, surgery, and midwifery? When the medical curriculum was lengthened by a year it was hoped that the additional time would be devoted to clinical work, but I fear that this is by no means always the case, for it is not uncommon for the student to take three years in passing his primary examinations, with the result that but two remain for the study of medicine and surgery. When I became a student of medicine I took to heart the advice of my teacher of physiology, William Bowman, and never allowed a day to pass without visiting the hospital; and though as a lad of sixteen I failed of course to appreciate the importance of all that I saw, yet I saw it, and I can carry my mind back now to cases seen and lessons learnt in the early fifties which are of service to me at the present time. But the student is practically forbidden to enter the wards now until he has satisfied the examiners in anatomy and physiology, which he may or may not do in two years, and there are but three years left for him to study totam rem medicam.

Far be it from me to decry the modern methods of teaching medicine in the wards of our hospitals. I believe that the care taken to induct every student into the mysteries of auscultation and percussion are beyond praise; and if with some teachers treatment is regarded as of secondary importance, at least the student has the opportunity of studying the vis medicatrix naturæ untrammelled under one teacher, and of watching the effects of every new drug upon the human system under another. But all this takes time, and so also the elaborate manipulations of the gynecological department, the researches of the pathological professor, and all the other teachings of the third and fourth years. And where, then, does surgery come in? Why, I consider myself fortunate if I can secure the regular attendance of candidates for a surgical diploma for the last three months, when their names are put on my list, and I subject them to rigid surgical cross-examination. But if I venture to refer to an illustrative case of last year, I find that no one present saw it or even heard of it, though at the time the whole surgical staff may have been in consultation upon it. How, I ask, is it possible for the student to see the serious surgical ailments which are not very common, such as aneurism, tumors of bone, tetanus, etc., if his attendance in the surgical wards is limited to a few months?

And yet there has never been a time of greater activity in operative surgery, not only among hospital surgeons, but among general practitioners, who, thanks to anæsthetics and antiseptics, undertake operations of magnitude which the hospital surgeon of the last generation would have hardly attempted. How are we to explain the apparent contradiction? In the first place, I am afraid we must allow that a great many mistakes in diagnosis are made, or rather, too often no attempt at diagnosis is made, but that an operation is undertaken to "clear up the case." That it generally does no doubt, but not always to the benefit of the patient. Then we must allow that, with unlimited time for the anæsthetic, the least skilful surgeon may hope to bring an operation to a conclusion more or less satisfactory to himself, and if he operates under fairly favorable circumstances, for his patient also. Lastly comes the enormously increased opportunity for the publication of a success at one of the numerous mutual admiration societies and in one of the medical journals of the day. Can we wonder, then, that young surgeons whose stock-in-trade of professional knowledge is of the smallest, blossom rapidly into operating surgeons in some special department, and try not unsuccessfully to prove that all is fish which comes to their net?

Still, gentlemen, the great foundations of the art and science of surgery remain undisturbed. Without a knowledge of anatomy, of pathology, and histology progress in surgery is impossible, and it is for those who hold the important positions of teachers in our great medical schools to insist upon a foundation of scientific and practical training being given to our students if they are to become the successful practitioners of the future. The growing tendency of the non-medical teachers of collateral science to regard their particular subject as the most essential for the medical student must be restrained, and the preliminary period of medical study must be cleared of many obstructions if the student is to have the necessary time to devote to the thorough study of those strictly medical subjects which will fit him to be a sound, practical, and at the same time scientific, physician and surgeon.

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SELECTIONS.

HAVE WE FOUND A "RADICAL" CURE FOR INGUINAL HERNIA? *

By W. B. DE GARMO, M.D.,

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When I was honored by an invitation to read a paper before your society, I felt all the more inclined to accept, because I had already experienced the pleasure of attending one of your meetings a few years before. At that time, I read a paper upon the subject covered by my present title, and I am glad to have the opportunity now afforded, of bringing before you a riper experience, fortified by an almost complete revolution in the subject treated of. In the grand advance of the science and art of surgery during these few years, the special surgery relating to the treatment and cure of abdominal hernia, has not been left in the rear. Perhaps, aside from that of pelvic surgery, no branch has made an equal advance.

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* Read before the Virginia State Medical Society, September 1, 1897.

The term radical cure, as applied to the treatment of hernia, has been badly misused for many years, being used as descriptive of operations that were more frequently failures than successes. We have had operations for the radical cure, injections for the radical cure, trusses for the radical cure, until the term had become such an abuse of the truth, that many of us had abandoned its use, as well as hope in its ultimate fulfilment.

WHY WERE FORMER OPERATIONS NOT RADICAL CURES?

We have little time in this age of rapid advancement to discuss past failures, except to ask why they failed, and our recent experience in this subject, has answered this question, and it now seems strange that we should have remained so long in the dark. It is a repetition of the old story of Columbus and the egg. After he had shown them how, they could all make it stand on end.

I take from my shelves a treatise on surgery, considered an authority at the time that my medical studies began, and find this sentence relating to the subject under consideration:

"The operations that have been devised for the radical cure of hernia are all founded on one of two principles, viz., the excitation of such an amount of peritonitis in the sac, or its neck, as to secure its obliteration, or the plugging of the hernial aperture by invagination of the integumental tissues." *

In view of such teaching, it is not surprising that there are still many middle aged practitioners, men who have been absorbed in the general practice of medicine, who do not realize that we have left that age of surgical darkness and entered one of broad daylight.

Almost without exception, the early operators felt that the essential step in obtaining a cure for hernia was the obstruction of the hernial sac, and every means consistent with the safety of the patient was resorted to, to accomplish this end. There was, however, the time honored dread of approaching the peritoneal cavity or any prolongation of its lining membrane, the peritoneum, which prevented anything really radical being done.

Up to the time of "Riesel," of Germany, few had ventured to make an opening further than the outer muscular layer of the abdominal wall, and after cutting off the sac at this point (considered a rather rash procedure), various methods of closing up the external, or superficial, abdominal ring were resorted to.

Riesel was undoubtedly on the right track when he advocated splitting the aponeurosis of the external oblique muscle, so as to expose the

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* Erichsen's Surgery, 2nd American Edition, page 791.

whole length of the inguinal canal, but his operation failed because of the subsequent defective closure of the parts.

Closely following this was the operation of my eminent townsman, Dr. McBurney, who went even further toward a final success, by opening the canal to the internal ring, clearing it of all foreign matter and tying off the hernial sac flush with the peritoneal surface. The subsequent steps in his operation, however, were fatal to its permanent success. Dr. Marcy, of Boston, had shown us the safety of the buried suture, and had insisted upon the necessity of restoring the obliquity of the canal, but the honor must be given to Bassini, of Padua, Italy, for finally supplying us with an operation apparently complete in its technique, and fortified by a simultaneous report of an unprecedently large number of cases; an operation, which has up to the present time, been performed upon a large number of persons, with greater safety and a larger degree of success, than any other in the history of the subject.

WHY THEN THE FORMER FAILURES AND OUR PRESENT SUCCESS?

To answer this intelligently, we must for a moment, consider some of the predisposing causes of hernia. In most works on this subject, you will find elaborate discussions upon elongated messentery, muscular relaxation, etc., which, doubtless, all have some element of truth in them, but in the light of recent experience, personally, I am convinced that there is an inherent defect in the muscular structures of man in what should form the posterior wall of the inguinal canal, and that operation which fails to overcome this defect can not be relied upon to effect a cure.

The lower borders of the internal oblique and transversalis muscles start from Poupart's ligament, and arching over the cord, find attachment in the public bone, but do not pass back of the cord. The cord, therefore, traverses an irregular triangular space, bounded above by the lower borders of the muscles named, outside by Poupart's ligament, and the base being represented by the public bone. Practically, the canal is unprotected at this point by posterior muscular wall. This triangle forms the weak spot in the anatomy of the parts, and was never taken into consideration in the older operations for hernia, but is now fully overcome, as will be demonstrated later on. That this muscular defect is a serious predisposing cause to the occurrence of hernia will, I feel sure, be admitted by those who have a large experience in the present method of operating, and, therefore, opportunity for observation.

Another cause seldom mentioned, but frequently met with, is the formation of fatty growths, either between the muscular layers, or, more frequently, just in front of the peritoneum, which slip into the canal and act as a dilating wedge. But, again, we have enormously enlarged veins.

connected with the cord, which, by their presence, tend to dilate the canal. These are merely mentioned as causes which were never reached or remedied by the older methods of operating. Besides these, there was always the more or less thickened sac, which left as a serous lining in the upper part of the canal, invited a recurrence of the protrusion.

All of the old operations dealt with hernia by remedying the defect as found at the external ring, leaving any defect that might exist in the canal; on the contrary, we now begin our repairs at the internal ring, after every abnormal structure has been removed.

Without further retrospect we will consider for a few moments our present methods of operating and the results obtained, then we shall be in a better position to answer the question asked by my title.

Among the members of your society, you number some of the most eminent American surgeons, who are, doubtless, familiar with every detail of this operation, and to such, even a brief recital of its technique may be tiresome, but I assume that the larger part of my audience are general practitioners, who have not seen it demonstrated, or who, at least, do not see why this should succeed when so many others have failed. Perhaps I will be pardoned, therefore, for rapidly going over it for their benefit.

A slightly curved line drawn from the anterior superior process of the crest of the ilium to the spine of the pubes, represents the location of Poupart's ligament. The incision for the Bassini operation should be about three-quarters of an inch inside of this line, extending from over the superficial abdominal ring about three inches upward and outward. The two underlying layers of fascia should be cut through, revealing the aponeurosis of the external oblique muscle, which will be recognized by its white glistening surface and the direction of its fibers. With the fascia, will be cut the superficial epigastric and superficial pubic vessels, which require clamping, and sometimes subsequent ligation.

At the lower angle of the incision, if it has been properly made, the superficial ring will now be seen, usually with the hernial sac bulging up out of it. A groved director should be slipped under the aponeurosis of the external oblique muscle, at the external ring, in the direction of the canal, and this tissue split by scissors or knife, to a point opposite, or a little above, the deep inguinal ring. You now have the entire canal exposed to view, and you proceed to lift the sac and the cord to which it is quite uniformly attached, out of its bed.

The separation of the cord from the sac, especially in hernia of the congenital variety, is the most difficult step in the operation, and some have failed entirely in its accomplishment.

The cause of failure has been, I think, due to making the attempt too low down. The separation must be accomplished almost entirely by the fingers, and I have found that, clearing one place on the sac surface and rolling the sac in one direction between the thumb and fingers, while the cord and fascia are held between the thumb and finger of the other hand, is the easiest way to accomplish it, and that the middle of the canal is the best point to begin.

The hard and wiry feel of the vas deferens should always be borne in mind, as this will lead to the location of the cord in cases of doubt. Having obtained complete separation at one point, little trouble will be experienced in stripping the cord free up to the peritoneal surface and as far down as may seem advisable.

The sac should be opened in every case, as only in this manner can you be certain that it does not contain adherent omentum, or possibly, intestine. One operator has been so unfortunate as to tie a loop of bowel in the neck of the sac by neglect of this precaution. I am in the habit of having the assistant tie the neck of the sac (with catgut), while I hold one or more fingers in its neck, thus avoiding possible protrusion under the ligature.

After tying, the sac is cut off outside the ligature, and its fundus stripped out entire, if this can be easily done. If, however, the sac is very large and closely adherent, that part which occupies the scrotum may be left in place, that only, which passes through the canal being removed. Having removed the sac, any other foreign body present can be taken away, and this should include all small growths of fat, which are very common, and prevent a subsequent matting together of the various layers.

The opening of the canal and clearing it of all structures that do not belong there, is, unquestionably, a great advance, but it certainly would fail, as it has, were it not for the step in the operation now to be described. Mention has been made of the relation of the lower border of the internal oblique and transversalis muscles to the canal, and they will now be plainly seen going just above the cord to their attachment to the pubic bone, and it will as readily be seen what a weak place in the anatomical construction of the parts this is.

The parts are reconstructed in the following manner: The cord being held up out of the way, a blunt curved needle, threaded with Kangaroo tendon, is passed through the lower borders of the internal oblique and transversalis muscles well up toward their attachment to Poupart's ligament. The needle is now carried under the cord and through Poupart's ligament. This suture, when tied, brings the borders of the muscles named, closely around the cord at the upper angle of the wound, and the muscles are now brought in contact with Poupart's ligament all of the way down to the pubic bone by continuous suture. We now place the cord upon this new muscular posterior wall, which never existed before, and stitch the divided apōneurosis of the external oblique over it, the skin being closed over this. This differs from Bassini only in the suture material, he using silk instead of the buried animal tendon. But as the latter proves fully as efficient and less liable to form a sinus, it is preferred by most American operators.

WHAT THEN HAVE WE DONE?

1st. Cleared the canal of the hernial sac and of any other foreign body that may have been found there.

2nd. Constructed a new posterior wall, by carrying two muscular layers back of the cord, which are not found there in the normal subject.

The treatment of the case subsequent to the operation is, usually, very simple. All possible precautions to secure asepsis have been observed, and it is expected that primary union will result. After closing the skin by catgut, and dusting aristol over the parts, a good firm bandage is applied and this is not disturbed for ten days, unless there is elevation of temperature, or pain, indication that all is not going well with the healing process.

In the two hundred and fifty cases reported by me before the American Medical Association, at Philadelphia, last May, there was primary union in two hundred and seven. In the fifty-five children under fourteen years of age, there was failure to obtain primary union in only one instance, which clearly indicates that in childhood, the ability to resist infection is much greater than in adult life.

In speaking of the result of the operation I shall draw upon my personal experience only. This, however, is fully in accord with the author of the operation and other operators.

One honor I believe I can claim, and that is the largest number of hernia operations, by any method, without a single death. Up to the present time, I have done two hundred and seventy-eight Bassini operations upon patients of all ages and conditions, and that without any dangerous symptoms following in any case.

The ages of the two hundred and sixteen patients upon whom the two hundred and fifty operations were performed, already tabulated, were as follows:

Under 14 years	55
Between 14 and 40 years	98
" 49 and 60 "	47
" 60 and 80 "	14
Over 80 years	2

Conditions at time of operation:

Right inguinal	154
Left inguinal	96
Double inguinal	34
Scrotal	93
Reducible	178
Irreducible	55
Strangulated	17
Omentum excised	55
Testicle in canal	9
Ovary in canal	3
Time of discharge:	
By 14th day	114
“ 21st day	198
“ 28th day	210
Over 28 days	6

One patient, a physician, operated upon for a good-sized hernia, left the hospital on the eleventh day, and, the same night, took a train for Detroit.

Two important points remain for consideration. The dangers attending the operation and the permanence of the cure. Besides my own two hundred and seventy-eight cases, I can, from five of those who have operated most extensively by this method, collect reliable results in over one thousand cases, which give only three deaths, and at least two of these can scarcely be attributed to the operation.

In recurrences, Bassini had seven out of his first two hundred and fifty cases. I have had seven recurrences, three of which have been operated upon, and as the special cause of recurrence was removed by the second operation, there is every prospect of permanent cure, leaving only four actual failures. Even in these four, the patients are in better condition than before the operation. Only two have sufficient recurrence to require the application of a truss. The others are wearing tight belts.

My answer then, to the title of this paper, is that we have found a "Radical Cure" for inguinal hernia. If any man on this floor wishes to argue against this conclusion, he has for his use the entire history of this subject up to the advent of this operation, because they were all failures. But I am sure that even that mass of evidence will not stand up before that which can be adduced in favor of the Bassini method.

PLACENTA PREVIA.*

By PAULUS A. IRVING, M.D.,
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While you may think I have selected as the title of my paper a rather old and trite subject, one about which possibly but little new matter has been advanced of late years, yet it seems to me it is well for us at our annual gatherings to discuss and confer as to the best modes of procedure in the management of many diseases which are termed commonplace, for they are the ones that oftener confront the practitioner, and certainly there is not one in the catalogue of diseases that requires as sound discriminating judgment and as skilful management as Placenta Previa. As this is a kindred subject and one as to its treatment that pertains to the paper of my friend Dr. Glasgow, who has so ably opened this discussion, at the suggestion of your secretary and with your consent, Mr. President, I will offer what little I have to say at this stage of our proceedings. My apology for presenting this subject for your consideration is that I have recently passed through a trying ordeal with a most serious case, and one that gave me great concern for several months, which I was fortunate enough though, to bring to a successful termination, saving both mother and child.

Placenta Previa is one of the forms of Dystocia which we meet with but rarely in our daily obstetrical work, but when it occurs the life of the mother, as well as that of the child, is often at stake, due to the great tendency and often the presence of exhausting hæmorrhage. The Placenta in a normal case of conception is attached at the top or fundus, or laterally to the sides near the fundus of the uterus. Fortunately, Placenta Previa is a rare complication; according to the most accurate statistics it occurs in one to one thousand labors, or, as compiled by others, in one to fifteen hundred labors. It is oftener met with in hospital than in private practice, for the reason that hospital patients, coming from the lower strata of society, seem more prone to this trouble for reasons that will be given further on. While this disease can appear as a complication at any age of child-bearing life, yet most cases occur from the thirtieth to the fortieth year.

The placenta, as is well known, in a normal conception is attached to the fundus or high up on the sides of the uterus; it is termed previa when it fails to attach itself to this portion of the uterine wall, but is attached to the lower segment of the uterus, to that portion of the uterus which not only during pregnancy, but after labor sets in, is stretched and distended by uterine contractions, the result of inhibitory muscular activity and stimulation. If the placenta is attached to the lower zone of the uterus or in the lower uterine segment, as termed by some, during gestation as the fœtus grows and the uterus enlarges and increases in size for the accommodation of the fœtus, why, owing to the peculiar abnormal implantation of the placenta, it often becomes detached in part, and gives rise to a more or less flow of blood, due to the placenta being torn away from its attachment. It is demonstrated that in most, if not all, cases of this form of Dystocia that Placenta Previa "originates from the development of the placenta within the reflexa of the lower pole of the ovum" (Hoffmier). Another theory advanced is that it is that of primary implantation of the impregnated ovum low down in the uterus, or even over the osinternum (Hart); or "that it is the primary grafting of the ovum in the inferior third of the uterine canal" (Winokel). It is further asserted that this condition is most apt to develop in women who have had some old chronic inflammation of the mucous lining of the womb, resulting in a diseased or abraded mucous membrane. "The Hypothesis I would advance is that the human ovum can graft only on a surface denuded of epithelium, and that it does not graft in Fallopian tubes, but in some part of the uterus removed by menstruation, hence if the ovum does not meet with any connective tissue surface until it has passed low down in the uterine cavity, why then some form of Placenta Previa will happen" (Hart).

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* Read before the Virginia State Medical Society, August 31, 1897.

VARIETIES.

For convenience of description, this form of Dystocia has been described as Complete and Lateral.

Complete Placenta Previa is where the os-internum is covered by the placenta, or, as designated by Schroeder and others, "a central implantation of the placenta." It is conceded that this form is rarely seen.

Lateral Placenta Previa is where the placenta is attached to the side walls of the uterus extending down or nearly to the os-internum, in some cases overlapping the os. This form of Placenta Previa is much oftener seen, and when you feel the placenta plugging the os, as it often appears to do, after there is sufficient dilatation for your finger to pass into the cavity of the uterus, you may at first think you have a central implantation, when in most cases it is attached laterally and simply a portion of it hangs down apron-like where it has become detached.

Barnes in discussing the varieties of Placenta Previa, divides the uterus into what he terms zones, viz.: (1) The superior or Fundal zone; (2) the Equatorial or Middle zone; (3) the Inferior or Polar zone. These zones or divisions of the uterus for the more intelligent consideration of the subject are separated, the one from the other, by imaginary, not anatomical, lines. The superior zone is the location of the placenta in a normal pregnancy. The equatorial or middle zone is the seat of lateral Placenta Previa. The inferior or polar zone is the seat of the central variety, or central implantation of the placenta.

Lusk, in my judgment, makes the most sensible and rational division of this subject, viz.:

1. Placenta Previa Centralis.
2. Placenta Previa Partialis.
3. Placenta Previa Lateralis or Marginalis.

In the first form you feel or detect on digital examination where the os is sufficiently dilated the placenta, and nothing but the placenta, directly over the os.

The second variety is where you detect the placental membrane as well as the placenta hanging down in some cases over or within the os, which can be pressed or diverted to one side.

The third is where the placenta is attached low down in the uterus, but does not extend as low as the internal os. This is the most favorable form of this disease, and the one from which better results follow.

CAUSES.

The real and exact cause has never been settled. The condition, though, that tends to this abnormal location of the placenta is oftener than otherwise an old endometritis of long standing, accompanied with a free leucorrhœal discharge; again, where changes have taken place in the mucous lining of the uterus from an imperfect involution, resulting from a too early getting up after confinement with all of its accompanying uncomfortable and distressing symptoms, which are so often seen in the poorer classes, necessity preventing their lying in as long as is needed for the uterus to regain its normal and proper size. Also a relaxed, enlarged or misshaped uterus, allowing the ovum to descend too low in its cavity before it attaches itself or becomes inserted. Where a woman has a free leucorrhœal discharge, whatever the cause of this excessive flow, it often carries the ovum low down into the uterus before it attaches itself to the uterine walls.

For the reasons above given, this trouble is more likely to occur in multiparæ or those women who conceive late in life, the per cent. being two or three to one according to some authorities; four to six as recorded by others. Another cause as given for this condition is where abortion has begun and for some reason it is arrested and the villi became attached low down in the uterus in the inferior zone, and at this point fœtal development continues.

Spiegelberg, in writing about Placenta Previa, rather predisposing abortion, says: "Owing to the loose vascular connections of the placenta and to the higher blood pressure in the placenta when inserted low down in the uterus, any shock is liable to cause rupture of its blood vessels and detachment." Maintaining, also, that shock affects the lower portion of the uterus oftener than the upper during the first months of pregnancy, arguing in this way for the above reasons that premature labor is more common in the early than the later months of gestation.

Placenta Previa has a tendency to repeat itself, the cause that produced it in the first instance not being removed makes it likely to occur again. Twin pregnancy it is claimed has a tendency to develop this condition, also fibroid tumors and cancer of the uterine walls.

CLINICAL FEATURES.

The first and most characteristic clinical feature of Placenta Previa is hæmorrhage, unaccompanied with pain, which may appear at any time during the latter months of gestation, if not after labor has begun, or just as the child is expelled. In many cases the first warning is a gush of blood and an exhausting hæmorrhage, generally without pain, though in some cases I have seen excruciating uterine contractions follow the hæmorrhage, which usually checks as suddenly as it comes on, followed by more or less symptoms of shock, depending on the amount of blood lost. Again hæmorrhage may be induced by exercise, stooping, straining at stool, fright, anger, any cause that brings about nervous excitment; the act of coitus has brought on hæmorrhage under these conditions. When hæmorrhage is due to such causes it is termed unavoidable hæmorrhage. While in both lateral and central implantation we are apt to meet with this symptom only during the latter months of pregnancy, when, owing to the growth of the child and the corresponding increase in the size of the womb, portions of the placenta are pulled away from their attachments, thus leaving a bleeding surface, the mouths of the blood vessels being left open with nothing to plug or compress them, and hæmorrhage results; the amount depending on the extent of the uterine surface denuded.

I affirm that while hæmorrhage is oftener met with during the latter days of pregnancy, generally from the thirty-second to the thirty-sixth week, yet it may be the first warning of impending labor, and uterine pains only assert themselves after the hæmorrhage is checked. If the hæmorrhage is exhausting and has been followed by shock and collapse, the uterine pains, though present, may be so slight and inactive, that inertia of the womb may result, so that manipulative and instrumental interference has to be resorted to in order to terminate the labor.

Two theories have been advanced as to the source and cause of hæmorrhage in Placenta Previa. Most of the authorities claim that the source of the hæmorrhage is uterine, others that it is placental; while it seems to me it might be both, yet, from my observation and reading, I lean to the uterine source of hæmorrhage.

Now, as to why the bleeding oftener occurs towards the close of gestation, there are also two views held by different authorities, many of whom are distinguished and advance most plausible theories for their side of the argument. One is that the lower segment of the uterus grows and develops faster than the placenta, which causes its detachment, and we have uterine hæmorrhage, which I believe is the true explanation; the other is that the placenta grows more rapidly than the uterus and pulls away from the uterine walls, leaving a denuded placental surface, and we have placental hæmorrhage. As I said above, the authorities are divided as to the correctness of these two different views, and it remains for some one in the future to demonstrate which of these theories is the correct one, and which theory we shall accept as the real and true cause of Placenta Previa hæmorrhage.

Hæmorrhage which is the chief and only practical symptom of this trouble comes usually without pain or warning. I have seen though the hæmorrhage followed by profound shock and excruciating uterine pains and contractions, which were only relieved by morphia hypodermically. My limited experience is, that this hæmorrhage when it once occurs, returns at stated intervals, and usually with some regularity. In some cases there has been a slight "show" for days at a time, then again practically all the time, just enough for the woman to wear a napkin, yet the hæmorrhage has a tendency to return, and usually with increased power and danger to the patient. The certain and only positive evidence of Placenta Previa is the sense of touch, which can only be demonstrated when the os is sufficiently dilated to admit of a digital examination and you feel the soft sponge-like placenta. It is not wise to resort to this diagnostic evidence unless you are ready to meet all emergencies and to deliver your patient at once. The cervix is much thinner, more richly endowed with blood vessels, which is the case also of the lower portion of the uterus and vagina. At this stage you can generally diagnose the type of the disease you will have to contend with, whether complex or lateral, or central, partial or marginal.

PROGNOSIS.

Every woman in whom this condition is recognized should be watched with eternal vigilance, for the death rate is high, both of mother and child, and when both are saved, all parties concerned deserve congratulations, and especially the plaudit "well done, good and faithful servant" should be given the attending obstetrician, who appreciating the danger ahead, calmly but courageously lies down to sleep at night with his armor on, anxiously waiting, fearing that every ring at his bell or knock at his door will bring him the summons that Mrs. ——— is bleeding to death. The danger is great not only prior to and during labor, but just as the child is born. Secondary hæmorrhage also is likely to result several hours after labor, due to loss of contractile power of the uterus, hence every woman should be watched for several hours after labor that is complicated with Placenta Previa, for many a patient has sunk into fatal syncope after delivery, brought on by profound shock combined with a weak heart, the result of excessive hæmorrhage. Sepsis also adds largely to the death rate. The maternal mortality ranges from twenty to twenty-five, fœtal mortality from fifty to sixty.

TREATMENT.

In considering the treatment of this disease only principles can be laid down, as each case must be a guide to itself, for you will find that many symptoms peculiar to each individual case arise that will demand special consideration, and which may modify to some extent at least any line of treatment that is advocated.

In the beginning I lay down this broad proposition which I feel confident no one will gainsay, that the life and welfare of the mother should first be considered and secondly that of the child. If one has to be sacrificed it must be the infant. In the conduct of a case of Placenta Previa, if it is possible, tide the pregnant woman over to the seventh month, and then if it is necessary for the mother's welfare, produce premature labor. In many cases this has been done and the lives of both mother and child have been saved, for at this period, unless the mother's system has been reduced and impoverished by sickness and disease, the child is what we term a viable age; many of whom, though born feeble and but poorly developed, have been reared to a strong vigorous manhood.

Should hæmorrhage appear, whether excessive or not, at any period of pregnancy, advise rest and quiet in the recumbent position, unload the lower bowels with a hot water enemata, direct a hot vaginal douche daily, to which you may add any of the antiseptic solutions, such as Borolyptol. Creolin, Carbolic Acid, etc., give black haw in teaspoonful doses every three or four hours, to which I often add from five to ten drops of ergot to tone up the uterus, and use opium in some form, usually a morphia tablet one-eighth to one-fourth grain to soothe and quiet the patient, and continue it in safe doses until you get its soothing and quieting effect on the general system as well as that of the uterus. If despite your efforts to control hæmorrhage it continues and you see the woman's strength failing, the nice and difficult question to decide is, when you are justified in producing premature labor, for if allowed to go on nature often terminates this condition with less risk certainly to the mother, and often to the child, than if manual interference is resorted to. Of course the extent of the hæmorrhage, the strength and condition of the mother, and the age of the fœtus, all are points to be considered in determining the line of treatment that you will pursue. Often after a severe hæmorrhage, accompanied with considerable shocks, when the physician arrives the hæmorrhage has ceased, he realizes how rapidly and how well nature will repair this loss. By proper treatment the woman will soon regain what she has lost, hence it is a nice point to decide whether to go ahead and precipitate labor, or wait, therefore I affirm each case must be a guide to itself, and it resolves itself into simply a question of judgment.

On the other hand, if on arriving at patient's bedside and the hæmorrhage is still going on and the woman's strength is flagging, you then will be justified in resorting to heroic measures. I would first of all tampon the vagina as tightly as I could with the aid of Sim's speculum, for it can be done much better with the speculum, using for the tampon small balls of absorbent cotton, of course aseptic cotton if it can be procured, and pack the vagina tightly. Just here I want to say that when you know you have a case of Placenta Previa your obstetrical bag should be fitted and ready, with every instrument and appliance that you may need for a rapid, aseptic, and instrumental delivery. In addition, you should keep at the patient's house those things needed in case of great emergency, for you can't tell what hour they will be needed, and badly needed, for the mother's life may be sacrificed by the delay in sending for them. The tampon usually can be left in from two to twenty-four hours, the pains and hæmorrhage being your guide as to when to remove it, unless the hæmorrhage persists in spite of it, but even if it does this can't be much if the vagina is tamponed properly, I mean tightly. But should the hæmorrhage start up afresh after the tampon has been in situ for several hours, you may then know that dilatation of the os has been effected, so then remove your tampon and proceed to deliver your patient as rapidly as possible; rupture the membranes, this facilitates labor; perform podalic version or use the forceps, if necessary; resort to that mode by which you can most quickly empty the uterus and check the hæmorrhage.

Suppose you meet with a case where the os does not dilate readily and the hæmorrhage persists in spite of the tampon, then you must resort to rapid dilatation, using a steel dilator at first until you can get your finger into the os-uteri, which is the best and safest dilator to use, and then stretch with Barnes' dilators sufficiently to admit your hand. If the placenta is adhered on the margin of the os or laterally, as soon as the head is engaged the hæmorrhage will usually cease, so break it loose from these attachments or carry it to one side, rupture the membranes and allow the head to engage. If the placenta is really a central implantation, why you are justified in tearing, if necessary, through it, break up its attachments and deliver the placenta first and then endeavor to engage the head or whatever part is presenting, that it may act as a plug or compress and stop the bleeding. Of course all this manipulating in the vagina must be done as aseptically as possible, or rather the operator must use clean hands, clean dressings and clean instruments.

The tampon is not looked upon with favor by some authorities, they claim that it is slow, that it increases the risk of infection and that there is danger of concealed hæmorrhage. They advise you to watch with vigilance and wait for something to happen, leaving nature to slowly dilate the os.

I am firmly of the opinion that if a tampon of aseptic material is used and the vagina is douched with an antiseptic solution and the operator's hands and instruments are clean there is but little danger of infecting the woman, and certainly dilatation of the cervix will be hastened by the presence of the tampon; and should hæmorrhage occur or continue it has a tendency to clot the blood, which by pressure has the effect of checking the hæmorrhage.

CASE.

To illustrate, Mrs. B. conceived about November 20, 1896. Not until the second month was she aware that she was pregnant, when she suffered slightly from twinges or nausea. To go back through to her former history and habit during her married life; her first pregnancy was a trying one, labor long and difficult, had to be delivered with instruments, resulting in a ruptured perineum, which was not properly repaired, followed by Endometritis.

The second pregnancy was complicated with Placenta Previa; no tendency to hæmorrhage until toward the close of gestation, but she went to full term and was delivered without any trouble. The third pregnancy resulted in an abortion about the third or fourth month. The fourth pregnancy was also an abortion. In the fifth pregnancy she went to full term and was delivered of a small child; nothing unusual about this labor. The sixth conception, when she became my patient, resulted in an abortion about the fourth month; she made a good recovery.

The seventh conception resulted in the birth of a dead infant about the sixth month, from which she recovered without any untoward symptoms. I give this meager and incomplete history to show the tendency and by it to prove the causes and theories I have advanced as evidence of this complication, Placenta Previa.

I come now to the eighth conception of my patient, which occurred about November 20, 1896. At the second month she had a hæmorrhage, not enough to weaken or alarm her or to give me any concern, but a decided hæmorrhage which by rest and quiet soon passed off with a slight flow for several days. At the third month there was another hæmorrhage with a slight discharge for several days, which repeated itself with increased force at the fourth month, with but little flow between these dates, which was readily controlled by rest in bed for a few days, the patient doing fairly well for the next month, being practically confined to the house and most of the time to her room. Appreciating the danger she was in, I advised absolute rest and quiet, which she religiously observed.

At the beginning of the fifth month I was hurriedly called to see her one evening, found her profoundly shocked, cold and clammy, with a feeble pulse; she had had a profuse hæmorrhage, just one such (which was brought on by nervous excitement from having to correct and reprove one of her children who had disobeyed her) and suffering intensely when I reached her bedside with severe uterine cramps, alternating with an excruciating pain in the top of her head, which I regarded as reflex. With morphia one-fourth, atropia one one-hundred-and-fiftieth hypodermically for the pain, and strychnine one-thirtieth hypodermically, and milk and whiskey by the mouth and rectum, and hot water bags to her feet, which were cold, she rallied in about an hour. On digital examination I found the hæmorrhage had checked as suddenly as it came; hardly enough discharge in the vagina to stain my finger, cervex only open enough to admit the end of my index finger, which though, I found soft and pliable. I gave her Fluid Extract Viburnum Prunifolium in teaspoonful doses every three hours, and morphia as indicated, directed perfect quiet and a liquid diet of milk and Bovinine, which had formed the greater part of her diet for several months, hoping all the time that labor would set in and terminate my anxiety, but such it would not do. I made preparation to meet every emergency, made my tampons of absorbent cotton, had antiseptic solutions ready (creolin, carbolic acid, bichloride mercury tablets, etc.). My obstetrical bag was fitted with every instrument that I thought it was possible to need, even to Gondell's steel dilator for rapid dilatation, sterilized water in abundance, aspiration needles, syringe and apparatus for transfusion, when indicated by loss of blood, to be used in the rectum, deep cellular tissues or in the cephalic vein, depending upon the urgency of the case; also ergot, ergotol, strychnia, morphia and atropia, ice, hot water, chloroform and whiskey; all of which I kept in her house ready for use for two months. She soon rallied, spent a good night, and, while weak the next morning, was comfortable, with no sign of hæmorrhage.

I made as you will see every preparation to induce premature labor, yet she seemed so comfortable and I was so anxious to tide over to the seventh month, when I hoped the child would live, that I decided to watch and wait. I left a most competent nurse with her, whom I directed to tampon the vagina and send for me without delay should hæmorrhage begin again. She rapidly made blood and in a few days was doing well. A flow about as much as appears during menstruation made its appearance in a few days, and continued more or less, not enough to weaken her though, until the sixth month, when she had another hæmorrhage, not so severe as the one that occurred at the fifth month, which was readily controlled. She continued to do fairly well during this, the sixth month of conception, the flow continuing more or less, enough for her to wear her napkin, stopping at irregular intervals for twenty-four to thirty-six hours, and beginning again. This state of affairs continued until July 1, 1897, when she had another hæmorrhage, though not severe, which was readily checked. On examination I found the cervix for the first time dilated, and open and dilatable. I could readily feel the placenta, and plugging or attached to the os-internum, but hanging directly over the os. Fortunately labor set in, dilatation continuing, no hæmorrhage. Labor progressed favorable, but slowly; in a few hours the head descended and the bag of waters formed, at which time the placenta must have been shoved to one side, for it no longer could be detected. After dilatation was complete, which I eased down with a little chloroform, because the pain was severe and wearing, I ruptured the bag of waters and labor then set in in earnest, and in a short while she was delivered of a very small and puny infant with only two expulsive pains. As the child was expelled there was profuse hæmorrhage, so I as quickly as possible, with one hand on the abdomen kneading the uterus, took away the placenta in clots, the uterus being filled with them. The uterus contracted down firmly and well, and no evidence of hæmorrhage appeared thereafter. No douches, vaginal or uterine, were used; the patient and bed were simply kept scrupulously clean. She made a good recovery in about a month, with no evidence at any time of sepsis, and is now doing well for her, having always been frail and delicate.

The infant was poorly developed, puny and very thin, succeeded in making it breathe after a minute or two, immediately it was wrapped in absorbent cotton an inch thick, over this several layers of flannel, and was put in a room the temperature of which was kept at eighty degrees, and fed upon condensed milk, giving it about two drachms properly diluted every hour and a half. This, of course, has been modified as the child grew and developed, and it is now doing well. The mother was practically in bed for three months and lived on a liquid diet all this time.

I expect to be criticised for the conservative management of this case, and feel sure many will say I ran great risk to the mother and that I should have produced premature labor certainly at the fifth month, yet the result in saving both mother and child is the endorsation I offer as the judgment displayed, and I come back to my original proposition laid down in discussing the treatment of Placenta Previa, that each case has to be considered from its own peculiar environments and the treatment resolves itself into a question of judgment at last.

The uniqueness of this case is that it so well illustrates the causes and theories advanced and the picture drawn of this disease. The chief clinical feature of interest I claim for it is, that hæmorrhage began the second month of pregnancy, which is very rare, and I at this early date diagnosed Placenta Previa.

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PROCEEDINGS OF SOCIETIES.

BRITISH MEDICAL ASSOCIATION.

Proceedings of the Section on Public or State Medicine at the Sixty-fifth Annual Meeting, held at Montreal, Canada, August 31 to September 4, 1897.

WEDNESDAY—SEPTEMBER 1.

The section convened at 10 o'clock, A. M., in the lecture room of the Redpath Museum, President E. P. Lachapelle, M.D., Montreal, in the chair.

Among those present from the United States were Drs. Benjamin Lee, of Philadelphia; C. O. Probst, of Columbus; Felix Formento, of New Orleans; Henry D. Holton, of Brattleboro; Liston H. Montgomery, Arthur R. Reynolds and N. S. Davis, Jr., of Chicago; Lt.-Colonel Alfred A. Woodhull, U. S. A., and Murray G. Motter, of Washington, D. C.; G. D. N. Hough, of New Bedford, Mass.; E. C. Rivers, of Denver, Colo.; Wolfred Nelson, of New York, and J. C. Shrader, of Iowa City, Iowa.

Dr. Lachapelle, after extending a hearty greeting to the section, read a resume or synopsis of his paper entitled

THE PROGRESS OF SANITATION IN CANADA.

The speaker analyzed all the sanitary legislation in Canada from the time of Louis XIV. to the present, and the hygienic measures under the French régime, 1603 to 1763. Thus in 1667 the King of France established for the Civil State a system of registration of births which is still in force in the Province of Quebec. The same law appertains also to baptisms, marriages and burials. In 1667 the question of food was considered and as a first step in this direction the inspection and price of bread was decided upon. In 1707 meat had to pass inspection regulations equivalent to our modern stamping. In 1748 there were regulations relative to foundlings until they were 18 or 20 years of age.

Following this he spoke of the typhus fever epidemic of 1795 in Ireland, which demanded the urgent protection of the colony, and vessels coming from infected ports and the manner in which they were inspected and quarantined. From 1815 to 1821 the government appointed medical vaccinators and granted prizes for a memoir on the advantage of vaccination, etc. In 1832 hygienic measures received a stimulus by the appearance of cholera and a quarantine station was established at Grosse Isle. In 1849 more precise laws were adopted, because of a second invasion of this disease, and since the Confederation (1867 to 1897) positive systemization has prevailed in the provincial governments throughout Canada.

Regarding Federal sanitation, all matters relating to foreign countries and to commerce are of Federal jurisdiction and accordingly sanitary laws were passed at Ottawa in reference to immigration and quarantine, the prevention of contagious diseases in animals, the suppression of food adulteration, the compilation of statistics, etc.

In the first year of the Confederation (1868) the Federal government enacted an "Immigration Act" destined to be completed (1871) by the "Quarantine Act," and rendered most effective subsequently. Mention was made of the eight well organized quarantine stations of the government, viz., Grosse Isle, Quebec; Halifax, Nova Scotia; St. John, N. B.; Sydney, Cape Breton; Hawkesbury, Nova Scotia; Chatham, N. B.; Charlettetown, Prince Edward Island, and William Head, B. C., including a detailed description of how the various staffs of these ports see to the inspection and disinfection of vessels, to the vaccination of passengers, to the detention in quarantine when required, etc.

Regarding contagious diseases in animals, the federal station (1884-1896) provides for the quarantining of exported animals and for the repression of epidemics among them.

The "Adulteration Act" appoints food analysts, to whom any person may submit samples; it also has some provisions respecting milk supplies and other foods (1884-1889).

As to provincial sanitation and municipal authority regulations the law varies in different provinces. Certain provinces, such as Manitoba and British Columbia, possess special organizations, while in the Northwest Territories and Prince Edward Island there are no provincial boards of health, although there exist sanitary laws investing extended powers in the Lieutenant-Governor in council and in the municipal authorities. In a word, hygienic laws in the Northwest Territories are adapted and elastic as occasion requires. Ontario and Quebec are perhaps the best managed Provinces. The Provincial Board of Ontario was founded in 1882; that of Quebec in 1886. These two boards have enabled public hygiene to make the greatest progress in Canada. In Ontario compulsory vaccination of all children over four months of age has been provided for by the statute and school teachers may require a certificate of vaccination from pupils. The Provincial Government has the entire control of vital statistics (1896), the compiling being done by the secretary-treasurer of each municipality. Here also the Food Act is very explicit in cities. In the Province of Quebec vaccination is obligatory by statute in four cities, but any other municipality may render it compulsory. The keeping of the registers of the civil state is still, as in the days of Louis XIV., confided to the care of the clergy of whatsoever denomination, and it is from these gentlemen that each month the Provincial Board of Health receives the certificates establishing the cause of death and those reporting the marriages and births. From these reports the Board compiles its statistics. The Provincial Board has power to make regulations to supplement in a certain measure the federal laws. In relation to the prevention of contagious diseases, hygiene in factories, sanitation in general, including the suppression of nuisances, etc., the Quebec Provincial Board has issued permanent and very precise regulations, and has formulated in them a standard of effectiveness that must be attained by the municipal boards. Apart from these permanent regulations it has, besides, the eventual powers of making special regulations during the prevalence of epidemics.

In both Ontario and Quebec the Provincial Boards may exercise the power conferred upon the local boards if such local boards neglect or refuse to perform their duties.

In Ontario the interpretation and the enforcing of the "Factory Act" is left entirely to the inspectors.

The approval of plans of aqueducts and drainage devolves on the Provincial Board in the Provinces of Quebec and Ontario. In Quebec the law regulates the disposal of the dead and enacts that the sites for cemeteries shall be chosen by the Provincial Board.

Speaking of other Provinces briefly: New Brunswick (1887) and Nova Scotia (1893) possess somewhat identical organizations, the Lieutenant-Governor in council having the right to legislate on sanitary matters, but only in Nova Scotia can the Lieutenant-Governor exercise his veto. The sanitary laws of New Brunswick (1887-9) very much resemble those of the Province of Quebec, as they confer on the Provincial Board the power to make regulations respecting the prevention of contagious diseases, the inspection of food and of insalubrious industries, drainage and the repression of nuisances. The registration law (1887) provides for the publication of vital statistics. In Nova Scotia, the authority of the Provincial Board with regard to contagious diseases is simply advisory. It may, however, during an epidemic, substitute itself for inefficient municipal boards. Manitoba (1893) admits of a special organization which seems to suit the needs of a country whose population is disseminated over a vast area of territory. Its Provincial Board is composed of five members, a veterinary surgeon and four physicians. For the needs of local administration, the four physicians are titled inspectors, and the Province is divided into four districts. In each district the authority of the inspector is supreme. British Columbia has (1895) a Provincial Board which, according to the statute, seems to be only advisory, but which nevertheless has established very complete and very effective regulations; for instance, it has a measure which surpasses those of any other Province, prohibiting the discharge of sewage into any river. Prince Edward Island has no Provincial Board. It has a good law respecting vaccination, and by its "Health Act," local boards are appointed in two cities, and the Lieutenant-Governor in council is empowered to appoint a board of health in each county. In the Northwest Territories a law relating to hygiene was passed in 1882. Its means are only preventive. The Lieutenant-Governor can hinder any person within the Territories from leaving a locality in which there exists a contagious disease. He can also delay at the frontiers any suspicious outsider. He can divide up the Territories into sanitary districts, and appoint medical health officers if he judge such measures expedient.

The regular order of scientific business was then proceeded with.

Dr. P. H. Bryce read a paper,

HOW FAR SHOULD MANDATORY MEASURES GO IN DEALING WITH: 1, MEASLES; 2, WHOOPING COUGH; 3, TUBERCULOSIS; 4, LEPROSY; 5, CHICKENPOX.

A brief analysis of his paper is as follows:

In Ontario the law, so far as it relates to chickenpox, is a dead letter on account of the mildness of the disease. Regarding measles, in 1894, sixty-five cases died in the entire Province of Ontario; in 1895, forty-one cases; in 1896, forty-three cases.

The writer assumed that the public is not prepared to be quarantined for this disease, as the gravity of the disease will not warrant it. Notwithstanding this, he recommended the following rules for school children for measles or whooping cough: 1. Daily notification to the health office, by the school authorities, of every absentee. 2. The truant officer should visit the home of the child immediately, for investigation. 3. A visit by a medical man to diagnose the disease if any exists. 4. Compulsory notification by the household (this will readily corral the disease, and practical isolation be made at once). 5. Notification, by the teacher, of any further cases of disease occurring in the school, and their immediate removal. 6. Home isolation by means of placards. 7. Disinfection of the house. 8. Closing of the school as a last resort.

Regarding whooping cough, it is contagious long before "cough" has shown itself, or where spasmodic coughing takes place.

As tuberculosis causes more deaths than any other disease, insist on notification of all cases of this kind being made to the health office. Insist on the regulation of the tubercularized, viz., to remain at home, to remain away from factories, etc. Establish hospitals or homes or sanitaria throughout the Province for them.

Leprosy cases need to be isolated.

Dr. C. O. Probst said that we are in the dark as to the cause of whooping cough, yet we can prevent the spread of the disease. He called attention to the fact that in the eleventh census of the United States, 9,256 cases of measles died, 8,432 cases of whooping cough, fifteen cases of leprosy, while 102,199 died from consumption. It is difficult to prescribe mandatory laws in the control of measles and whooping cough, as public opinion is against it. Although we should require notification of whooping cough, yet we have no isolation hospitals for such cases. Apparently, then, notification may not amount to a great deal. Closing the public schools does not protect the children unless the disease is epidemic. The prevalence of these diseases is due to their mildness. He advocated double notification referred to, placarding the homes of the sick and other domiciliary measures. In leprosy, he was in favor of the National Government establishing a place for the segregation of lepers. In tuberculous subjects we can not, unfortunately, isolate these cases. Their sputa should be disinfected. In tuberculosis of lower animals we can enforce mandatory measures in the sale of unsound meat and impure milk, or slaughter the diseased animals when found so by the tuberculin test. There is a close connection between breathing impure or foul air and consumption. Capitalists and others must be made to perform their duty, and those in authority should be held responsible.

Dr. H. Handford of England was not in favor of mandatory measures for these mild types of disease. In England many think there is too much legislation already. We need in this branch of medicine in England a Minister of Health.

In scarlet fever the removal of a case can be made compulsory by a magistrate's order, but in his city of 250,000 inhabitants he does not think that such an order has ever been issued. He advocated education and persuasion of the parents on the part of the attending physician, that their confidence may be gained and a case removed voluntarily. Health officers are inclined to be looked upon as officious. He was, however, in favor of mandatory or compulsory measures regarding the disposal of impure milk and impure meat; as in England there are laws governing this, also governing tuberculosis. Leprosy is a clinical curiosity in England. He thinks that what applies to whooping cough applies practically to measles, although in measles the mortality varies greatly. In cities of 43.05 deaths in every 1,000 cases occur, while but 6 cases in every 1,000 cases of measles die in country districts; in some cities in England, however, the mortality may reach 49. Notification in England is required by law, and it is a double notification both by parents and teachers.

Dr. William Oldright of Toronto agreed in part with the last speaker. He thinks we lose sight of much of the educational idea, but he thinks compulsory measures are also necessary. He said that whooping cough is more dreaded than measles in some families and cited a number of illustrations in support of his statement. Regarding tuberculosis, as a usual thing it is not properly dealt with. Rooms at health resorts frequented by tuberculous patients should be more thoroughly disinfected. The same rule holds good regarding the disinfecting of rugs, carpets, etc., in sleeping cars and hotels, which should be washed oftener; as a rule, this is rarely done.

More isolation hospitals should be established for scarlet fever and diphtheria cases, and with these comes in the mixed infection class; when we come in contact with such a case, where one variety is exposed to the other the case may become more complicated. If placarding is done it should be thorough and not inefficiently. Placards on milk wagons, stating that the dealer deals only in pure milk, is a strong inducement for the people to patronize such firms.

Dr. Ebenezer Duncan, of Glasgow, thinks tuberculosis is the most important of these diseases to combat. He believes the greatest amount of good can be obtained by increasing the cubic space for breathing pure air and that overcrowding should be condemned. If thorough ventilation is carried out there is not much chance of infection; good feeding should go with this, then notification in case of death. He did not think it was possible to carry out effective mandatory measures with regard to measles and whooping cough.

Dr. Thomas Carr, of Braintree, England, urged the necessity of disinfecting the sputa of tuberculous cases. Regarding measles and whooping cough we are not yet ready to isolate these cases in a large district. In 80 per cent. of the towns in England where notification has been tried, it has been abandoned.

Dr. Alexander Johnson, of Glasgow.—If we attempt to remove cases of measles or whooping cough to isolation hospitals, the plan is likely to fall through and then we are placed in an unfavorable light. Public sentiment is with the people in this matter. Measles is scarcely at all contagious after the eruption has appeared. In whooping cough it is more infectious when the secretions or sputa are abundant and such cases should by all means be isolated.

Dr. Wolfred Nelson, of New York.—Leprosy is in no sense contagious. I have seen the disease in the Island of Trinidad and the Isthmus of Panama, and have known sisters of charity to nurse the disease twenty-six years and be in constant attendance, and not a single case developed among them. It is simply a newspaper sensation to state that this malady is contagious, and as intelligent medical men we should add no additional burdens to these unfortunates by sending them awav from their former surroundings and family.

Dr. Benjamin Lee, of Philadelphia, replied to the last speaker by saying or inquiring, If leprosy is not contagious, in what manner do you account for its propagation from person to person? The health authorities of Cape Colony have recently published an exhaustive report on the prevalence of leprosy in that country, in which they strongly sustain the doctrine of contagion. Negative testimony may wipe out positive testimony, although not reliable.

Dr. John A. Malcolmson, of Middleborough, Eng., thought we could advance public health matters best by educating the people concerning all kinds of disease.

Dr. Felix Formento, of New Orleans, concluded the discussion by referring to the cases of leprosy he had seen in his city. The disease seemed to be transmitted by heredity or contagion.

Dr. James T. Neech, of Atherton, Eng., read a paper on

THE PERIOD OF INFECTION IN SCARLET FEVER.

He thought six weeks too short a period for the specific poison, infection or microbe to cease to multiply, even though desquamation may be complete. Infectivity can be retained in the fluids of the body indefinitely. Complete elimination in the shape of spores is not secured by the emunctory organs. According to his experience the minimum period of contagion is four to eight weeks; the maximum period, six months; the average is thirty-nine to sixty-five days. He has known cases discharged from hospitals, as in the above periods of time, convey the disease. He believes the infection is stored up in the secretions of the nose, lungs, lymphatic glands, etc. He had obtained bacteriologic results in hospital cases at the expiration of eight weeks, and even thirteen weeks, and he cited one hospital case of 181 days' convalescence wherein the disease had been conveyed. In all cases he advocated that the clothing should be sterilized by steam.

Dr. Johnston, of Glasgow, in the discussion, thought eight weeks for hospital cases a short enough time in which a patient could be deemed free from the germs of the disease, and even then his clothing should be steamed and disinfected. Notwithstanding precautionary measures, 2 per cent. carried infection away with them. He cited an illustration of this kind, that had been in the hospital for three months and then discharged cured, that infected and spread the disease through an entire household. He suggested that these cured cases should be required to sleep alone for two or three months.

Sir James Grant, of Ottawa, said that formerly, when there was no sewerage he could trace the origin of scarlet fever to defective drainage. If he should attend a case of scarlet fever he should not think of attending a case of obstetrics short of six weeks. His idea is that the kidney gives the most trouble. The period of infection is not thoroughly defined yet, for the disease may be propagated for a very indefinite period.

Dr. Malcolmson recited a case of infection of scarlet fever that had been retained in a hospital fourteen or fifteen weeks that spread the disease after being discharged as cured. The result was that the health authorities were sued and a heavy judgment was obtained.

Dr. Adam Frederick John Mickle, of Christ Church, New Zealand, said: It is not desirable to keep children in a town hospital for too long a period after convalescence from scarlet fever. As soon as convalescence is well established, say in three weeks, and sometimes earlier, they should be removed to the country, as complete recovery will be much quicker in a pure atmosphere; other means can be used to eliminate the poison or infection, such as hot baths; these will hasten the period of complete recovery. Regarding the danger of attending midwifery cases, while attending a case of scarlet fever, I do not think the danger so great as it is usually believed to be. During a very severe epidemic of scarlet fever lasting nine months, which I had to contend with about twenty years ago, I also attended midwifery cases in considerable number and never had any bad results. If asepsis is carried out there is no danger. In some instances the children were laid down with scarlet fever in a room with the mother, where she was confined, and yet I did not see a bad result in a single case of midwifery during the whole epidemic, notwithstanding the scarlet fever swept the children like a plague in the first few weeks of the epidemic. This was doubtless due to the insanitary condition of the town, which was practically without drainage, and the houses being small and on the Scotch system of flats, there was much overcrowding, and lastly, people will not submit to too much isolation.

CADAVERIC FAUNA

Was the subject of an illustrated paper read by Dr. G. de N. Hough, of New Bedford, Mass. He divided the said fauna into: 1, workers of the first stage of putrefaction; 2, workers of the second stage; 3, workers of the third stage; 4, consumers of the dried tissues; 5, consumers of the remains of the other workers. These parasites were given in their respective orders of diptera, coleoptera, lepidoptera, etc.

UNDERGROUND ZOOLOGY AND LEGAL MEDICINE.

A study of 150 disinterments with additional experimental observations, by Dr. Murray Galt Motter, of Washington, D. C. The author had experimented and made his observations on bodies that were buried from one year and eleven months up to seventy-one years. The subject is a new one and his paper, medico legally, very interesting. He proved the same kind of beetles, flies and other insects and germs had been found upon bodies whether they had been buried in June or in January, which would disprove the theory that a body supposed to have been buried in either of these months was in reality buried six months subsequently or previously, as the case might have been. He found tetanus germs active after the body had been buried three years.

(*To be continued.*)

ABSTRACTS.

SOME SURGICAL SINS.

By EMORY LANPHEAR, M.D., Ph.D.. LL.D., St. Louis.

There are a number of "sins of omission, as well as of commission" which surgeons may well pause to consider. Let no man lay flattering unction to his soul and say: "This is not for me." Wait and see! Profiting if there be aught to censure in your work.

First Sin.—Operating on Hopeless Cases.—There are some—and they are called surgeons—who will undertake almost any kind of an operation regardless of the prospects of cure or failure, nay, even of life or death; and sometimes without consideration of the question of their ability to do the work properly.

The incentives are: (a) to get a good fee; (b) to have the credit of operating, especially in rare or difficult cases; (c) to obtain control of the influential patient from a conservative physician who, recognizing the hopelessness of the case, is doing all he can to ameliorate suffering and prolong life, but persistently refuses entreaties of patient and friends to "do something;" (d) ignorance of the real condition.

The objections to operating in cases well known to be hopeless are: (1) It is wrong. The doctor who will operate upon a case merely to secure a good fee, knowing perfectly well that death will follow, or that no benefit will be gained, is no better than a common thief! And the surgeon who will, under like circumstances, operate before a class merely to demonstrate the technic of the operation and show his own adeptness, is little better. Yet I have heard a "professor" in one of the schools of St. Louis confess to this practice. (2) It brings discredit upon surgery. Therefore, I plead for (a) more careful examination before operation, (b) more thorough diagnostic and prognostic skill, (c) more judgment in the selection of cases; for I am charitable enough to believe that most operations upon hopeless cases are performed, not through desire of notoriety or gain, knowingly to the detriment of the patient, nor yet as an attempt to gain an advantage over a rival, but through ignorance of the actual condition and of the prospects for recovery after operation; indeed, I, myself, must plead guilty to having been indiscreet in the selection of cases in the enthusiasm of my early work. Verily, wisdom increaseth with age—and experience. (3) It prevents other patients with similar disease from submitting to operations clearly indicated. I recall an instance in point; I was called to Nebraska to remove a cancerous breast; it was too far advanced to cure; operation could but hasten death and increase suffering; I refused to do anything; after my departure the doctor amputated the breast without entering the axilla (!) and the woman died in six weeks. A lady upon a neighboring farm whom I saw at the same visit had a scirrhus of the breast just developing, almost consented to extirpation, waited, and was lost; she was "scared out" by the death of her friend; another life sacrificed through ignorance.

The diseases in which this mistake is most liable to be made are (a) appendicitis with perforative peritonitis; (b) cancer of the breast; (c) hip-joint disease; (d) malignant affections of the uterus; (e) severe, acute trauma where waiting a few hours would determine the question of life and death.

SECOND SIN.—Delaying Opinion as to the Gravity of a Disease.—This applies particularly to cases which the attending physician knows full well tend to a fatal termination if not subjected to early operation; yet he hesitates to tell the patient the character of the disease and the necessity for surgical treatment—waiting, tinkering, giving medicines, encouraging the patient with false hopes—hopes, he admits, based upon bubbles thin as air; but still waiting, ever waiting—for what, God alone knows. I never have been able to fathom such men, to determine why they temporize, too often until too late. One would think them few, but this is not true. Such men are numerous—I meet them quite frequently. Only a few months ago a physician brought me a case of cancer of the uterus from the Indian Territory, far advanced, stinking, yet still operable. He was well informed, a graduate of one of our best colleges; had treated the case for some months recognizing its malignancy all the time. Yet he begged me not to tell the patient what was the trouble. He had informed her that a simple little operation would have to be performed for her "ulceration of the womb"—a "simple little operation;" hysterectomy! Well, I told her of her condition and of the prospects with and without operation, and let her decide. She submitted gracefully, and was sent home in three weeks relieved of present danger, possibly cured. The doctor was in an agony of indignation for a time, but regained his equanimity when the patient did not denounce him for his deceit, and he forgave me for my frankness, on account of the prompt recovery of his patient. Such episodes are not at all uncommon. They are met chiefly in connection with (1) uterine fibromata, (2) cancer of the uterus, (3) cancer of the breast, (4) amputation—for septic wounds, osteomyelitis, tuberculosis, etc., (5) appendicitis, and (6) opening of purulent accumulation as (a) empyæma, (b) psoas abscess, (c) cold abscesses of the neck, (d) pus in the joints, etc. I might dwell much longer on this topic but—*verbum sat sapienti.*

THIRD SIN.—Failing to Operate in Depressed Fractures of the Skull. —Until quite recently it has been the teaching of schools to leave all fractures of the skull severely alone, unless some grave symptom demanded operation, regardless of depression. This was because there was more danger (before the days of antiseptic surgery) in opening up wounds of the head than in leaving them alone. And in these degenerate days, when even a Joseph Lister takes a pocket case from his pantaloons, removes a knife, automatically dips it in carbolic solution and proceeds to operate, I am not sure but the old doctrine is still a pretty good rule to follow—for some operators. Indeed, I have recently seen some American surgeons who pretend to be moderately clean (more of which later) that I would not trust inside of my head. But, presuming that every community now has at least one physician sufficiently familiar with antisepsis and asepsis to keep his professional brethren of the neighborhood from picking his instruments out of the solution to examine their construction (a thing that not infrequently has happened to me, and not always in the country, either), I must insist upon the future observance of Agnew's rule: "Every depressed fracture of the skull should be operated upon regardless of the amount of depression and irrespective of pressure symptoms." The reason for this is that nearly 50 per cent. of serious injuries to the frontal region, when not trephined, terminate in epilepsy or insanity; nearly all of which sequela can be prevented by operation at the time of accident. It is not good practice to wait for the nervous system to arise and then operate. The way to cure traumatic epilepsy is to prevent it; also, traumatic insanity.

But many men do not do this, especially in the country—why, I do not pretend to say. Timidity, perhaps, possibly a fear of censure if the patient dies after operation; and of legal complications in accidents of homicidal origin. The latter often deters surgeons, oftener than justifiable, in all sorts of injuries—not to the head alone. And there are probably other reasons. Be that as it may, I know that many cases of depressed fracture are still left untrephined, for I meet such cases quite often and recent ones, too. Doctors are too afraid of urging operations in such cases in opposition to the wishes of relatives and friends, and continue in charge of the case in spite of the whisperings of conscience that a wrong has been done in not insisting upon operation or quitting the case. I have little respect for the man who has not the courage to live up to his convictions. But the possibility of loss of dollars and of custom, and of influence and of friendship, even, is a powerful salve for a bleeding conscience; and so the skull isn't raised, and the doctor who knew better but didn't dare, remains in charge, and an incurable epilepsy or an insanity is put to the debit side of his account for settlement in the great hereafter! Let us see. Here is a case in point.

P. G. was 52 years of age at the time of the operation. Some four years before he came under my care he was kicked upon the forehead by a mule. There was a marked depressed fracture of the frontal bone in the median line just at the margin of the hairy scalp; but as he fell into the hands of a "conservative" surgeon who believed that a depressed fracture should not be operated upon unless profound symptoms of pressure are present, he was not trephined, because he was unconscious but a few moments, and very soon went about his business as if the injury were of no consequence. If a progressive modern surgeon had been called, who believed that every depressed fracture of the skull should be subjected to operation at the time of injury, however slight the depression, and irrespective of pressure symptoms, the most disastrous consequences might have been averted; for very soon a most remarkable change occurred in the patient. Before the accident he was an exemplary husband and devoted father; a strong opponent of intoxicating liquors and a Christian gentleman; noted for his integrity and excellent character. But soon it was seen that his character was totally changed—he deserted his wife and sought the companionship of prostitutes; he was so abusive to his children when at home that they fled from him in terror; he became so addicted to the use of whisky that he soon wasted his property and sunk to the level of the drunken sot, as often found in the gutter as in bed; he cursed and swore and seemed to lose all sense of manhood and decency—he was obscene, vulgar and debased. This condition continued for a period of more than three years, with occasional intervals in which it was thought he was going to "reform;" finally, after a long debauch, suicidal and homicidal impulses became predominant, and at last the relatives consented to operation. Upon removing the depressed bone a large fragment of the vitreous plate was found projecting through the dura an inch or more into the frontal lobes. The superior longitudinal sinus was crushed and filled with organized blood clot, and much of the middle frontal convolutions showed signs of pressure, but no decided degeneration. There was little shock and no meningitis following this extensive operation. A marked quietness instead of constant restlessness was the first change noticed in the mental condition; the countenance became more serene, the voice less coarse. As convalescence progressed, it was found that the patient was more like his former self—he greeted his wife and children with a pleasant smile and kind words, instead of scowls and curses. And when bodily health was completely restored he became his old self once more; gentle, affectionate and trustworthy. This improvement was not temporary. More than two years have elapsed, and his mental condition is as good as before the accident. He recalls the incidents of his mad career as one remembers a vivid dream, shuddering at the horrid recollection and trying to forget the terrors of the past in the pleasant realities of the present.

Now here was a triumph of surgery, to be sure. But, supposing this man had not fallen into the hands of one who could restore him; or that he had died while yet in his iniquity? If what the theologians tell us be correct, would not the doctor who failed to do his duty be held responsible for this soul's eternal torment? But—religion aside—from a purely ethical standpoint, has a doctor the moral right to commit this "third sin?"

Fourth Sin.—Pretending to be Clean, but Failing.—Every one now knows the inestimable value of asepsis; most surgeons pretend to practice it; nearly all fail. The clean (ideally, surgically, truly clean) surgeons of America do not number a dozen! I know them—I mean the leaders of the profession, not the hewers of wood and tillers of soil, not the common, everyday workers like you and me, but the men to whom we look for guidance. Why, I saw a "professor of surgery" in one of the leading schools of America, not long since, making a thyroidectomy before his class; his spectacles fell into the wound; he picked them out, deliberately readjusted them and went on about his work as if nothing had happened; and in reporting the case subsequently he wondered why he had pus in the wound—yet he would be scandalized if I were to assert that he doesn't practice aseptic, modern surgery! He thinks he does. It is just that kind of men who commit this "fourth sin." They are the men I am after, and they are not confined to America, either. England has but few men who are doing ideal work, and the continent is nearly as bad—or it has changed wonderfully since my last visit. The points of failure are:

(1) Dirty finger nails. Lawson Tait's remarks that "the high mortality rate in abdominal surgery found in the work of German surgeons

is easily accounted for—they don't clean their finger nails," is applicable to the methods of some surgeons not Teutonic. Too many operators simply scrub their hands for a few minutes, wipe them upon a towel, wash in bichloride solution for an instant, and then proceed to operate.

Doctor! Does that apply to you? Now you know well enough that, after carefully scrubbing your hands for several minutes, you ought to dry them and closely trim the nails; then scrub with much soap and muscular energy for five minutes more and then sterilize them by (a) immersion in alcohol for at least two minutes, and then washing in sublimate solution, or (b) washing in saturated solution of potassium permanganate until hands and arms are of a mahogany color, decolorizing in a saturated solution of oxalic acid, and finally rinsing in sublimate solution, 1 to 1,000, and then in boiled water. You know you ought to do this before every laparotomy (abdominal section), trephining, thyroidectomy, resection of joint, etc.—but do you do it? If not, you are guilty and need censuring.

(2) Carelessness in touching clothing, table, pulse of patient, handle of pitcher, etc., during an operation, and then going on without further washing, is entirely too common with those who are perfect in their methods of preparation. One of America's greatest teachers, writers and operators—a man of whom we are all proud—has to be constantly watched by his assistant in regard to his department of the "fourth sin;" in his enthusiasm, he forgets his own precepts. "To err is human." There is some satisfaction in knowing that Missouri does not have all the sinners. We all should be more particular about contamination of our hands and instruments during capital operations. Eternal vigilance is the price of—asepsis.

(3) Carelessness in keeping and handling gauze. (a) It is too common a practice to keep dry gauze wrapped up in a paper which is hurriedly unrolled and handled with dirty fingers in cases of haste. If kept dry, a necessity with the iodoform variety, it should be rolled in aseptic, oiled paper, and placed in a tin box or glass jar which closes tightly, and occasionally resterilized by heat. Preferably, gauze should be kept in a glass jar, in sublimate solution, tightly closed, and a dirty hand never thrust down after a piece. I, myself, keep two jars—one for dressings for pus cases, of which I am not so careful; the other for operations and aseptic dressings—from which the pieces are taken by means of a pair of forceps, sterilized by passing through the flame of an alcohol lamp before insertion in the jar. (b) Gauze is too often cut with scissors not sterilized, especially in making dressings. I know a surgeon of more than local reputation who invariably cuts his gauze for dressings with scissors taken from his velvet-lined pocket case; and he is a teacher, too. But that does

not make it right. (c) Allowing assistants or nurses with suspicious hands to cut the gauze is just as bad. I recently saw a "trained" nurse in a hospital assisting at a removal of the breast; she was cutting gauze for "sponges"—moved the slop-jar under the table for the surgeon, handed the ether can to the anæsthetizeur, and continued cutting gauze without a thought of washing. Such cattle ought to be taught the value of the Hindoo proverb: "Cleanliness is the key to heaven."

(4) Improper selection and management of instruments. (a) Those without metal handles—so that sterilization by boiling or otherwise is impossible; (b) those with old-fashioned points—so that perfect cleaning is unattainable; (c) failure to scrub and boil all instruments immediately after operation in septic cases.

Fifth Sin.—Undercharging, in Order to Secure an Operation.—This is a sin too often committed by men who stand high in the profession; and this assertion is no idle figment of the imagination, either. As a young man, I am often compelled to bear the brunt of the sins of older men. For example, a physician in Northern Kansas made all arrangements for me to operate for an ovarian tumor; a banker's wife; fee agreed upon, $500. The patient arrived at the hospital during my temporary absence from the city; banker "accidentally" fell into conversation with another, an older surgeon, who remarked that $250 was his (or to be charitable, perhaps, he may have said the) usual charge for such an operation; banker, on inquiry of nurses, physicians, etc., found this man to be "the best surgeon in the city," and he was $250 "ahead;" I was $500 "out." Many such experiences have been mine; 'tis the fate of the young and the meek.

Hic fabula docet—but this isn't a fable, and really is not supposed to teach anything, except, perhaps, patience—which, like the poor, the young surgeon must have always with him.

Sixth Sin.—Stealing Patients from Other Surgeons.—This is more often the cause of hard feelings between surgeons than any other one thing, though, as it is done usually in such a genteel, "ethical" way there is no recourse left to the injured party, except to growl. Occasionally one, with more enthusiasm than discretion, "prefers charges"—which come to naught. But sometimes the biter is bitten. I recall an instance. I had made all arrangements to operate, in conjunction with a distinguished surgeon of Memphis, upon a Southern lady, who had a very bad pyosalpinx; now, in the city where she was stopping there was a surgeon who had made a number of sections without a death and was very ambitious to reach the magical fifty cases without a death, beyond which world-wide fame is supposed to lie; he, hearing of the case, managed to be introduced, and with an eloquence worthy of a better cause, persuaded her that he was the only man that could do the work well; he actually took her in his own buggy as he left, carried her to the hospital and operated upon her without any preparatory treatment, and without waiting to consult her husband, who had left the city for a day; the patient died a few moments after removal from the table; the surgeon "broke his record," received not a dollar, and made an enemy of the husband—but, as for me, I am his friend still; it was simply a little professional indiscretion, bred of enthusiasm on his part. But there are cases of genuine, pure, unadulterated stealing, "too numerous to mention," as the auctioneer says, and too vexatious to repeat.

SEVENTH SIN.—Representing Capital Operations as "Trifling."—In order to secure consent to operations some are inclined to belittle the danger, represent the work as "trifling," etc. Of course, I do not now refer to instances where there is good reason for not going into the details with a patient, explaining dangers to such a degree as to cause refusal to consent to an urgent operation. Such cases are quite often met; and the explanations must be made to the friends, not the patient. But to gross misrepresentation or, at least, to the deception of silence as to the dangers. Once I was guilty of this sin; it was several years ago—when I was just entering upon my surgical work. I had a "streak of good luck," had made some thirty or forty abdominal sections without any bad results, and I thought I could do anything in pelvic work; in my over-confidence I committed the "seventh sin," once more demonstrating the truth of Byron's words: "But ignorance must ever be a part of sin." A beautiful girl was brought to me from Southwest Missouri, with tubal abscess, double; she questioned anxiously if there were danger in removal and I assured her there was none. I removed the tubes, secure in my faith in my own works. I knew not that I tore into the rectum in separating adhesions; she died of peritonitis—never uttering one word of censure, but oh! that look upon her face as she passed into the realm of shades! Then I felt, indeed, the grievousness of my sin. I believe I shall never again operate upon a patient, except in emergency, without fully explaining the possibilities of the case with and without operation, letting him decide. And this is right.

Example teaches better than precept. One of our surgeons of international reputation had as a patient one of the wealthiest women in the city. Disease, ovaritis chronica. She was doing well under chronic treatment and would have remained a semi-invalid to the end of her days, but was ambitious to be well, as became a leader of society. The doctor was likewise ambitious—to be known as an abdominal surgeon, and so he tempted the woman with hopes of health. He said: "Removal of the ovaries is a very simple, little operation: some morning I'll bring my table, my nurse and my assistants and we'll take out these little troublesome things and make you well." "And is there no danger, doctor?" "None in the least, madam." And she consented. Oophorectomy was performed. The next day the doctor left town for twenty-four hours. Patient developed peritonitis, called another surgeon, and was told next morning she had but a few hours to live. She prepared a surprise party for her physician. When he returned to town he hastened to her home and there found some twenty or thirty ladies, the most prominent and influential in his practice, gathered at the bedside. The lady, reaching out her hand, seized his in a grasp not to be shaken off. Calmly, amidst profoundest silence, she spoke: "Dr. ——, I was a happy woman, an invalid to be sure, but happy in the love of relatives and friends, surrounded by all the comforts and luxuries that wealth can provide, not suffering much, though wanting health. I could have lived for many years in the peace and happiness of my family. But in an evil hour you came and sang the siren's song. You told me there was no danger. Now I have peritonitis—am dying by your hands. Do you deserve further confidence or success? No. I have called your friends to hear my last words to you. With a dying woman's breath I curse you. Go!" As she flung his hand from her he went—and his business went also. Need more be said?

Eighth Sin.—Keeping Patients too Long Under Chloroform.—'Twas the ambition of surgeons prior to 1846 to operate rapidly; he who could most quickly perform an operation was rated the best surgeon; in celerity was success and reputation. But, with the introduction of anæsthesia, the other extreme became more and more popular until to-day the whole city of New York does not contain a half dozen operators who insist upon speedy work even in the abdomen. Many, indeed, say: "It does not matter how long the belly is open if one is clean and the room is warm." Fatal error. Nowhere in surgery is haste more necessary than in abdominal and pelvic work; the man who can most quickly open the abdomen, do his work rapidly, yet well, and close his wound with least delay is the one who will have the best mortality rate, and in spite of the fact that he does operations that the slow man would not dare to attempt. The advice of a great operator, anent ruptured tubal pregnancy, is apropos: "Get into the belly as soon as you can, but for heaven's sake get out."

Beside the danger of long-continued exposure of surfaces liable to be infected, with consequent disastrous results, there is the greater one from the anæsthetic itself. We have been wont to ignore this; with immunity from pain and perfect quietness, we forget that at every operation, however small, in which chloroform or ether is given, our patient is literally carried to "the valley and shadow of death," and though he oft apparently returns in safety, in a few hours may pass beyond. "Shock" is usually due to (1) excessive hæmorrhage; or (2) prolonged anæsthesia, and many a death certificate has been filled in with "shock" when the real cause of death was too much chloroform or ether. Nor is this danger all. Even though the patient recover from the immediate influence, if he has been kept too long in complete narcosis the remote effects upon the nervous system may be profound and lasting. As Wheaton says: "There is little doubt that anæsthesia is responsible for this adding of weeks, months, and perhaps years, of suffering and woe, and that many a tortured nervous system lives to-day to testify to the truthfulness of this statement."

In doing our operative work we should, therefore, ever bear in mind "Ariston metron"—the golden mean—the middle course is best; not using undue haste at the expense of safety nor keeping our patient too long under the ever-pernicious influence of anæsthetic. Unwise speed is bad; "chronic surgery" is worse.—*American Journal of Surgery and Gynæcology.*

THERAPEUTICS.

TREATMENT OF PHLEGMONOUS AFFECTIONS BY DEEP INJECTIONS OF POTASSIUM OXALATE.

For some time Dr. G. Cavazzani, Chief Surgeon to the Venice Civil Hospital, has treated phlegmonous inflammations by injections of a 1 per cent. solution of potassium oxalate into the tissues around the morbid focus.

The idea of employing this substance in abscesses, for the purpose of combating the engorgement of the capillaries and the plastic exudation characteristic of this process, was suggested to him by the anticoagulant action of potassium oxalate on the liquids of the organism.

The theoretical reasoning, however, which has led Dr. Cavazzani to adopt this method of treatment is of minor importance in comparison with the encouraging practical results which he claims to have obtained.

He first employed potassium oxalate in the case of a woman, thirty-six years of age, who for two months had suffered from an abscess, involving the entire hand, forearm and lower half of the arm. The acute period of the affection had passed, but the limb remained stiff, swollen, red, hot and painful, although there were neither extensive purulent collections nor gangrenous patches. Under these conditions, ten injections of a 1 per cent. solution of neutral potassium oxalate were administered along the whole length of the affected limb, the liquid being pushed into the deep layers of connective tissue. In this manner, 2 cubic centimeters of the solution were injected. On the following morning, the swelling redness and pain were found to have considerably decreased. The injections were repeated at intervals of three or four days, and complete recovery was obtained within three weeks.

Since that time, Dr. Cavazzani has had occasion to prove the favorable effects of potassium oxalate in quite a number of phlegmonous affections, as well as in two women suffering from phlegmatia alba dolens, who recovered rapidly under the influence of from ten to twelve injections of 2 or 3 cubic centimeters in all, repeated twice in one case, three times in the other.

Among the cases treated by this method was one of inguinal abscess in a man, who had been operated upon for a hernia three days previously. Eight injections of altogether 2 cubic centimeters of the oxalate solution were administered at once around the inflammatory focus, with the result that all the alarming local symptoms had subsided on the following day, leaving only slight suppuration of two sutures.

Lastly, it is worthy of note that these injections of potassium oxalate have never, in Dr. Cavazzani's experience, given rise to the slightest symptom of intoxication.—*Medical Week.*

HABITUAL CONSTIPATION.

The causes of habitual constipation are: Heredity; the habit of suppressing the desire; unsuitable diet—too abundant, deficient in water, too easily absorbed, or insufficient in variety; sedentary habits (although obstinate habitual constipation may occur even in those who lead an active life); disturbances in the circulation (heart disease, mechanical pressure, pregnancy); displacement of the bowel; intestinal adhesions. The treatment is given as follows: (1) dietetic, (2) physico-mechanical, (3) medicinal. In the first method, such foods should be given as are known to increase peristalsis. The "physico-mechanical" includes suitable massage (often of great value), electricity, and enemata. The disadvantage of enemata is that ultimately small quantities of fluid do not suffice, and large quantities over-distend the intestine and become useless. Regular attempts at defecation with slight pressure are recommended. Medicinal treatment is discussed under two groups, mild and more powerful purgatives. An efficient rhubarb preparation is very useful. Calomel is very useful in children. Castor oil is unsuitable for constant use. Large injections of olive oil are of value.—*Berlin. Klinik.*

MISCELLANEOUS.

LIQUID AIR AND ITS USES.*

Air is the vapor of a liquid, and acts in its properties like the vapor of other liquids; for it liquefies at a pressure of 573 pounds per square inch, with the temperature reduced to —220 degrees Fahr., and upon gradual release of pressure commences to boil at 294 pounds pressure, with a falling temperature, reaching —312 degrees Fahr., when the pressure is entirely released, at which temperature it will maintain its stability exposed to the atmosphere for some little time, according to the quantity under trial, and holding its intensely low temperature by its own evaporation until the whole is evaporated.

The commercial production of liquid air is a very important discovery, and the future question of economy in motive power may be intimately associated with this liquid. Compressed air, at pressures ranging from 1,000 pounds upward, is conducted from an air receiver through a small pipe, is refrigerated to expel its moisture, and is then conducted into the apparatus which liquefies it completely, without the use of chemicals of any kind, and it flows from this apparatus in a stream about the size of a lead pencil (in the apparatus of Linde) into a glass insulated receptacle containing about two gallons. This receptacle was filled in a very short time. Of course, being in an open vessel, liquid air has no pressure, but its temperature is approximately —385 degrees Fahr., or 445 degrees below the atmosphere at 60 degrees Fahr. Inasmuch as it boils rapidly on the surface, owing to its absorption of heat from the atmosphere, it looks like carbonated milk on the surface, but upon dipping some of it out in a glass and observing its color through the glass, it has very much the appearance of ordinary water, and about the same weight.

Its temperature is very deceptive, for as it runs from the condenser one may allow it to trickle over the fingers for a short space of time, and it appears to have the atmospheric temperature. The truth, however, of the matter is that it does not come in contact with the fingers at all; the hand being something like 480 degrees warmer than the liquid, it throws the liquid into a spheroidal state and interposes between it and the finger a film of atmospheric air. The sensation is very much like pushing one's hand into a bag of feathers or into a mercury bath, allowing, of course, for the difference in weight between the mercury and the liquid air. If, however, the hand is immersed in the liquid a sufficient time to establish a contact, the flesh would be burned, the same as if it were exposed to 440 degrees of heat, measured above the atmospheric temperature. If a test tube of 1½ inches diameter, having a couple of pounds of mercury in the bottom, is immersed in liquid air, the mercury will be frozen solid in a few seconds, and may be hammered out and otherwise manipulated the same as lead. An alcohol thermometer of large size will be frozen instantly upon being immersed in the liquid.

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* Compressed Air, condensed for Public Opinion.

A tablespoonful of liquid air poured on about a fluid ounce of whisky will freeze it at once into flat scales, giving the whole the appearance and color of cyanide of potassium. This may be emptied out on a table, and will remain frozen in that condition for fully five minutes. One thing that impresses one is that, while all molecular motion is practically arrested at this temperature, the odor is perfectly distinct, showing that these particles which stimulate the sense of smell are active and independent of the temperature. A handkerchief of either silk, linen, or cotton, saturated with the liquid, will be charred and destroyed just the same as if it were put in an oven and browned, though no change of color is apparent. Its evaporation is quite slow, and it may be carried about for a number of hours in an open vessel without entirely disappearing. It probably represents a compression of about seven hundred atmospheres, and would, therefore, in a confined space and at 60 degrees temperature, represent a pressure of somewhere from ten to twelve thousand pounds to the square inch.

Liquefying air is not a new thing; it has been performed by exerting enormous pressure or by freezing air to an unusual degree, or by a combination of pressure with refrigeration. There are so many uses to which liquefied air can be put that scientists hardly know where its usefulness will end if it can be produced at a low rate of cost in commercial quantities. Among other advantages, air in the portable, cheap form of a liquid, as it passes back to its ordinary state, can be used for illuminating purposes by mixing its escaping gases with atmospheric air in certain definite proportions. Moreover, as a driving force in the way of detonators, or explosive material, to drive engines, liquid air is obviously a power that can be, under given conditions, profitably applied.

Fluid air costs about 10 marks ($2.25) for five cubic meters, reduced. The new method is the invention of Professor Linde, of Munich. It produces the liquid for 10 pfennigs (two and one-quarter cents) for five cubic meters, and it yields the product either as a gas or fluid, as one wishes. This is one of the most ingenious pieces of mechanism recently known; its chief feature is its economy of working, for it uses air to refrigerate air. After the pump has worked for a certain time, one turns a cock and liquid air runs out at a temperature of 273 degrees below zero.

In Professor Linde's method, an air-pump of five horse power condenses air to a pressure of 200 atmospheres; this air passes down a spiral tube and is let out in a chamber, causing great cold; then it rises and passes on the outside of the spiral tube, bathing it and thus cooling the new air that has been pumped into the tube to take its place. This cooled air follows on into the chamber, expands and again lowers its temperature, then passes on up around the spiral tube; but as its temperature has become much lower, the new air in the tube is still further refrigerated. This circulating process goes on, until the new air pumped into the tube reaches the expansion chamber at a temperature of 273 degrees below zero, when it drops into the chamber in the form of liquid. Thus the air, steadily cooled, is made to refrigerate the newly pumped air more and more, until the necessary degree of cold is attained. Another idea, which may or may not be an improvement, is to have the pump and all parts of the machine kept very low in temperature.—*Medical and Surgical Reporter.*

NEWS AND NOTES.

DIRECTIONS FOR PREPARING SPECIMENS OF BLOOD.

The skin covering the tip of the finger is thoroughly cleansed and then pricked with a clean needle deeply enough to cause several drops of blood to exude. Two large drops are then placed on the glass slide, one near either end, and allowed to dry without being spread out on the surface of the slide. After they have dried, the slide is placed in the holder and returned in the addressed envelope to a culture station, or mailed to the laboratory. The blank giving the history of the case must be filled out in full and forwarded with each specimen. The data thus obtained are for record.

DIRECTIONS FOR OBTAINING SPECIMENS OF SERUM FROM BLISTERS.

The shield (designed to protect the blister from rupture), is stripped from its protecting gauze and applied to the skin somewhere on the anterior portion of the body. The pieces of canthos plaster is then fixed within its center. After 10 to 12 hours, the shield is removed and one of the ends of the small glass tube accompanying the outfit is introduced into the blister. The tube, both ends of which should be open, should be held so that the end inserted is higher than the other, to allow the serum to run into it. After the tube has been nearly filled, it is removed and the ends sealed by holding them a moment in a gas flame. Care must be observed not to heat the middle portion of the tube, and thus coagulate the serum. The tube so prepared is then placed in the wooden box and returned in the addressed envelope to a culture station, or mailed to the laboratory.

A report on the result of the examination will be mailed to the attending physician on the following day.

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EDITORIALS.

PRACTICAL POINTS CONCERNING SYPHILIS.

A recent article in the Medical News by one of the best syphilographers of the country—Dr. R. W. Taylor—entitled "What conditions influence the course of syphilis," brings into prominence a number of facts more or less well known to those who have had much experience with the disease, as well as suggests not a few other things of considerable importance to those who will be called upon to manage specific cases. It has always seemed to us to be exceedingly desirable to first of all—after an exact diagnosis has been reached and demonstrated, impress upon the patient the fact that moral and social ostracism do not necessarily follow in the wake of the disease; and in the next place that there is no other disease more tractable, if the patient be willing to obey instructions, nor any other in which the effect of treatment is more easily demonstrable and more satisfactory. On the other hand, there is perhaps no other disease more destructive if it assumes a malignant type, and if it be not promptly and properly checked. The *materie morbi* has not been demonstrated with certainty, but is almost without question a specific microbe. That acquired syphilis can only follow the chancre as its initial lesion has long been accepted as a dogma of the disease. The pathology of the initial lesion is stated that such clearness by the author mentioned that we quote him in extenso:

"The initial lesion, from which the following facts were derived, was excised by me four days after its first appearance and eighteen days after the infecting coitus, consequently it presented the very earliest appearances of syphilitic infection which can be studied by means of the microscope. The chancre appeared as a minute red erosion, and consisted of a circumscribed mass of tissue resembling in structure ordinary granulation tissue, but under and beyond it, when examined microscopically, there was a very marked change in the condition of the blood-vessels. This change consisted in the distention of the perivascular spaces with small round cells. Nearly every vessel examined, both arteries and veins, was in this way enveloped by masses of small round cells, forming a sheath like a coat-sleeve around the arm. Besides this condition of the perivascular spaces there was a change in the endothelial cells lining the arteries and veins, which were swollen and seemed to be proliferating. In two or three places the process had gone on to such an extent as to produce thrombi of the medium-sized veins. Careful study showed that it was very probable that these perivascular cell-clusters were produced by the proliferation of the connective-tissue cells of the adventitia of the vessels or by the proliferation of the same cells, just exterior to the adventitia. The point deserving of attention, and it has been further demonstrated in the examination of chancres from other patients, is the extremely early and far-extending involvement of the blood-vessels. Although the primary sore was but of a few days' duration, very small, and under the microscope was of such limited and circumscribed extent, the blood-vessels were very extensively surrounded by cell investments at a considerable distance—fully an inch—from the ulcer. The microscope also showed how deep-rooted syphilis is at the very beginning of the sore, having extended along the perivascular lymph-spaces, and how futile it is, as experience has already shown, to attempt to abort it by excising the primary sore. Apparently, judging from the appearance of the vessels in this case, their involvement begins before the appearance of the sore. The microscopic analysis of this and other cases also shows how it is that the infection of syphilis travels from the primary sore and infects the body. The perivascular spaces are lymph-spaces which communicate ultimately with the inguinal glands, and the enlargement of these glands is undoubtedly due to the advancement of this cell proliferation along the perivascular spaces until it reaches them. The same process extends from the inguinal to other sets of glands or lymphatics.

The process of syphilitic infection, therefore, is one of constant growth and diffusion from the beginning. In the very first days of the existence of the chancre the poison is deeply rooted in the subcutaneous tissue; it is in a most active state and progresses along the course of the vessels until it reaches the body, and then infects the whole economy. In consequence it is very certain that excision of the chancre will not cause the abortion of the disease. Reasoning by analogy, we are warranted in assuming that this new, highly infectious, and rapidly growing tissue gives off, probably by means of its microbes, a poison which diffuses itself through the system.

We have then in syphilitic infection two orders of morbid changes: (1) the heterologous new cells; (2) the diffusible poison."

This then compels the belief—as statel in the quotation—that the excision of the chancre, its cauterization or any other method of disposing of it than by systemic treatment, is utterly useless. We are therefore under still heavier obligations to our patients and ourselves to make a diagnosis that admits of no question. The next fact of especial interest brought out in the article is that "there is a well-marked uniformity in the infectious quality of the virus, no matter from whom it may be derived, and that this poison may produce in some subjects a mild and in others a severe form of syphilis. It, therefore, logically follows that the benignity or severity of syphilis is determined by the condition of the individual." Keeping this in mind we will not be slow to bring our patients up to the best standard of health, besides treating the special disease for which the patient happens to consult us; more than this we must carefully enquire into the mode of living, the surroundings, habits, company and every attendant circumstance that may throw any light upon the way to improve the general condition of the patient.

The author goes on to say: "In a general way it may be stated that the larger number of persons who contract syphilis are those who are in average good health and have not grown old. In a smaller number the standard of health is less high, and in many patients certain morbid conditions exist which are due either to disease or bad habits, and which lower their power of resistance. Extended clinical observation shows very clearly that syphilis when untreated by mercurials runs a somewhat uniform course. Following the chancre, in due time the secondary cutaneous and mucous-membrane manifestations appear, and remain for a short or long period, and then they seemingly undergo, resolution. Next a more advanced form of lesions, such as large papules or papulo-tubercles, are observed, and these may in time wither, and later on—in months or years—true tertiary lesions may develop. The tendency of syphilis, luckily for mankind, is to expend its force on the superficies of the body, and it seems very probable, particularly in healthy subjects, that the lesions of the deeper parts are, in the main, due to various determining causes, such as traumatisms (bones, joints, tendons, and fasciæ), antecedent pathologic processes (liver, spleen, kidneys, intestines, and testes), and to a neuropathic tendency (cerebrospinal affections). When any of the abovementioned causes exist in untreated syphilis, the grave order of lesions may follow or coexist with the more superficial ones. Some of these mild cases, in persons previously healthy, sooner or later become grave, and even malignant, but in general the gravity or malignancy of syphilis is due to some inherent defect in the constitution of the patient, to some diseased condition, or to a lowered state of health due to privation or bad habits.

The pathology of early syphilis is revealed to us in the cell-infiltrations constituting the essential lesions which are distributed in a symmetrical manner over the whole body. With the evolution of this new growth a diffusible poison is developed which is carried throughout the entire system and gives rise to the various phenomena (fever, debility, emaciation, headaches, neuralgias, arthralgias, periosteal pains, splenic engorgement, and sometimes pleuritis). With the destruction of the newly formed cells by treatment the tangible lesions undergo involution and disappear, and in proportion to the completeness of their cure so does the poison secreted by them grow less and less in potency and quantity. When a perfect cure does not take place, some of these morbid cells remain (in all probability in little masses around the blood-vessels, and not stored in the ganglia), but as they grow old they lose their vitality and increase slowly, and, having lost much of their virulency, produce very little, if any, of the diffusible poison. This is what takes place in tertiary syphilis, in which the new growths are indolent, aphlegmasic, and show a marked tendency to localization and to asymmetrical distribution. The gumma is the direct and feeble descendant of the virulent round-cell infiltration of the secondary stage of syphilis."

The subject is always as interesting as it is varied, and we shall again consider other questions relating to it; the facility with which some excellent ones allow a patent syphilitic lesion to escape them when it happens to occur in "high places," reminds us of the fact that we must be ever ready to recognize and treat the disease—whether or not we see fit to make mention of it to the patient or friends—entirely independent of rank or person.

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